

February 1, 1877.

Dr. J. DALTON HOOKER, C.B., President, in the Chair.

The Presents received were laid on the table, and thanks ordered for them.

The following Papers were read :—

1. "On the Mean Directions and Distribution of the Lines of equal Barometric Pressure, and their Relations to the Mean Direction and Force of the Wind over the British Isles, &c." By J. A. BROWN, F.R.S. Received December 30, 1876.

[PLATES 11 & 12.]

1. *Mean Directions and Distribution of the Isobaric Lines.*

In considering atmospheric variations, it is always desirable to know, if possible, the mean values about which the others fluctuate: this appears to be especially the case with reference to the direction of the lines of mean barometric pressure and of the atmospheric currents. If any common law exist connecting the statical and dynamical pressure of the air, this will probably show itself with some precision by an investigation in which, all the cases (the observations of every day) being included, deviations from the law may be expected to neutralize each other, and the final results give absolute measures directly comparable with each other.

For any exact determination of the lines of equal barometric pressure it is essential to possess observations from stations whose heights above the mean sea-level are accurately known, and made with good instruments which have been compared directly or indirectly with each other. These conditions are well satisfied by the observations made at the Greenwich, Dublin, and Makerstoun Observatories in the eight years 1842 to 1849 (both inclusive). The barometers were all by the same maker, each having a tube of nearly 0.6 inch internal diameter; they were all compared directly or indirectly with the Royal Society's standard; and the heights of the cisterns were determined by levelling from the sea in each case. Under such circumstances the directions and intervals of the isobaric lines may be found with much more precision than from observations made at any number of stations where these conditions are not fulfilled.

In the following investigation I shall assume that, within the limits of the three stations, the mean directions of the *isobars* for each month,

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as deduced from the eight years' observations, may be represented approximately by parallel straight lines drawn on a plane chart.

The positions of the three barometers are as follow :—

	Lat.	Long.	Height above mean sea-level. ft.
Greenwich	51° 29'	0° 0'	159
Dublin	53 21	6 16 W.	18*
Makerstoun	55 35	2 16 W.	213

Taking the meridian of Greenwich for the axis of y , and the circle of latitude of Greenwich for that of x , represented by a straight line on the chart perpendicular to y (both on the same plane), we obtain the following equations for the isobars passing through Makerstoun (M) and Dublin (D):—

$$\begin{aligned} \text{M} & \quad . \quad . \quad . \quad y = ax + b_1, \\ \text{D} & \quad . \quad . \quad . \quad y = ax + b_2; \end{aligned}$$

and if y , x , b_1 , and b_2 be expressed in geographical miles, then from the preceding coordinates we have for the points M and D,

$$\text{M} \quad . \quad . \quad . \quad 246 = -77a + b_1, \quad . \quad . \quad . \quad . \quad (1)$$

$$\text{D} \quad . \quad . \quad . \quad 112 = -224a + b_2, \quad . \quad . \quad . \quad . \quad (2)$$

where the usual trigonometrical directions are employed.

If β_1 be the difference of barometric pressures (always for the mean sea-level) for Greenwich *minus* Makerstoun, and β_2 for Greenwich *minus* Dublin, these will be proportional to the perpendiculars from G to the isobaric lines through M and D; hence

$$\frac{\beta_1}{\beta_2} = \frac{b_1}{b_2} \quad . \quad . \quad . \quad . \quad . \quad . \quad (3)$$

From these three equations we find

$$a = \tan \theta = \frac{112\beta_1 - 246\beta_2}{77\beta_2 - 224\beta_1} = \frac{1}{2} \cdot \frac{\beta_1 - 2 \cdot 2\beta_2}{0 \cdot 344\beta_2 - \beta_1}; \quad . \quad . \quad . \quad (4)$$

so that, if θ be counted from the north through the east, south, and west (as for the directions of the wind), we find for the isobar passing through G and M,

$$\beta_1 = 0, \quad \tan \theta = \frac{+2 \cdot 20}{+0 \cdot 69}, \quad \theta = 343^\circ \text{ or } 163^\circ;$$

for the isobar passing through G and D,

* The height given by Dr. Lloyd is 24·4 feet above low water of spring-tides, from which I have subtracted 6 feet to reduce to mean sea-level, as at the other stations (Dublin Observations, vol. i. p. 386)

$$\beta_2=0, \quad \tan \theta = \frac{\pm 1}{\mp 2}, \quad \theta=300^\circ \text{ or } 120^\circ;$$

for the isobar passing through M and D,

$$\beta_1=\beta_2, \quad \tan \theta = \frac{\mp 1.20}{\mp 1.31}; \quad \theta=228^\circ \text{ or } 48^\circ.$$

The following Tables contain the monthly mean barometric pressure at Greenwich for the eight years, and the corresponding values of β_1 and β_2 .

TABLE I.—Monthly mean Barometric Pressure at Greenwich reduced to mean Sea-level, 1842–1849. (Add 29 inches.)

Month.	1842.	1843.	1844.	1845.	1846.	1847.	1848.	1849.
	in.	in.	in.	in.	in.	in.	in.	in.
January.....	1.082	0.851	1.069	0.882	0.847	0.918	0.996	0.949
February	1.057	0.651	0.676	1.021	1.025	0.963	0.692	1.285
March	0.923	0.935	0.886	0.975	0.831	1.066	0.679	1.093
April	1.091	0.861	1.174	0.870	0.763	0.828	0.763	0.692
May	0.955	0.836	1.119	0.885	0.951	0.936	1.097	0.938
June	1.070	0.871	0.984	0.945	1.034	0.976	0.812	1.039
July	0.990	0.996	0.922	0.939	0.925	1.092	1.005	0.958
August	1.037	0.988	0.847	0.899	0.946	1.046	0.892	1.010
September.....	0.885	1.188	1.052	0.974	0.994	0.997	1.003	0.937
October	1.025	0.778	0.734	1.021	0.688	0.976	0.818	0.918
November	0.775	0.893	0.864	0.750	0.997	1.080	0.961	0.919
December	1.184	1.424	1.066	0.834	0.877	0.868	0.983	0.973
Year	1.006	0.939	0.950	0.916	0.906	0.979	0.892	0.976

TABLE II.—Differences of the Monthly Mean Barometric Pressures at Greenwich and at Makerstoun in thousandths of an inch.

β_1 .

Month.	1842.	1843.	1844.	1845.	1846.	1847.	1848.	1849.
January.....	+256	+256	+135	+129	+219	+ 72	+ 30	+201
February ...	+206	— 90	+115	+ 73	+170	+ 96	+262	+226
March	+201	+ 34	+118	— 8	+188	+ 51	+ 88	+108
April	— 93	+138	+132	— 9	— 9	+136	— 70	+ 5
May	+ 95	— 20	— 99	— 55	+ 70	+103	+ 95	— 93
June	+ 75	+ 19	+125	+117	+100	+ 73	+106	+ 71
July	+ 94	+130	+ 66	+ 85	+140	+ 68	+156	+145
August	+ 84	+ 99	+127	+ 90	+ 26	+ 63	+135	+121
September...	+ 1	+ 20	+ 1	+ 95	+ 30	+159	+ 52	— 89
October.....	+106	+141	+103	+184	+144	+ 95	+ 32	+ 76
November...	+ 90	+184	+ 65	+192	+105	+200	+121	+152
December ...	+299	+223	— 71	+227	+ 35	+139	+203	+ 8
Year	+118	+ 94	+ 68	+ 93	+101	+105	+101	+ 78

TABLE III.—Differences of the Monthly Mean Barometric Pressures at Greenwich and Dublin in thousandths of an inch.

 β_2 .

Month.	1842.	1843.	1844.	1845.	1846.	1847.	1848.	1849.
January	+ 93	+ 97	— 5	+114	+160	+163	+ 36	+ 79
February ...	+165	— 80	+ 2	+ 33	+ 66	+ 7	+136	+102
March	+ 97	+ 95	+ 22	— 68	+119	+ 53	+ 8	+ 13
April	— 56	+121	+ 74	+ 3	— 55	— 5	+ 32	— 28
May	+ 89	+ 12	—110	—102	+ 38	+119	+ 44	+ 26
June	+ 30	+ 4	+ 70	+ 60	+ 67	+ 1	+ 73	— 15
July	+ 51	+ 24	+ 8	+ 56	+ 84	+ 14	+ 71	+ 61
August	+ 38	+ 70	+ 19	— 15	— 5	— 10	+ 84	+ 52
September...	— 27	— 20	— 13	+ 53	+ 18	+ 26	+ 26	— 57
October	— 41	+ 21	+ 81	+ 96	+ 89	+ 88	+ 27	+ 80
November ...	+119	+100	+ 74	+137	+137	+119	+ 18	+ 92
December ...	+166	+139	+ 40	+ 35	+ 52	+240	+290	+ 60
Year	+ 60	+ 48	+ 22	+ 34	+ 64	+ 68	+ 71	+ 39

Although we may assume, when observations for a considerable period of time are considered, that the isobars over the small space about the three stations may be represented by parallel straight lines, yet this assumption becomes less probable when the observations for single months are employed; since in these occasional cyclones may produce considerable irregularities. I have thought it desirable, however, to calculate the values of θ for each month by equation (4), in order to obtain some approximation to the probable errors of the directions deduced from the whole eight years' observations. The values of θ are given in the following Table.

TABLE IV.—Directions of the Isobars (θ) for each Month, counted from North through East, South, and West.

Month.	1842.	1843.	1844.	1845.	1846.	1847.	1848.	1849.
January	277 ⁰	276 ⁰	298 ⁰	236 ⁰	248 ⁰	186 ⁰	216 ⁰	275 ⁰
February	242	55	296	270	275	293	265	270
March	268	180	288	348	256	228	292	292
April	78	236	262	129	340	302	131	336
May	232	134	41	14	263	218	269	126
June	274	286	262	265	253	296	251	304
July	263	287	290	254	259	286	270	273
August	270	250	290	287	303	303	257	272
September...	242	320	342	262	258	289	267	75
October	307	290	243	261	257	233	239	224
November	210	263	219	245	210	259	289	258
December	262	257	133	289	203	196	205	269
Year	268	266	280	278	256	255	251	266

Variations of Yearly Means.—The yearly mean barometric pressure does not seem to vary according to any law (see last line of Table I.). The greatest pressure at all the stations occurred in 1842, and the next

highest in 1847 and 1849, the least pressure being in 1848. There is, however, an appearance of systematic change in the yearly mean values of β_1 , β_2 , and of θ , which may be noticed on account of its partial coincidence with the decennial period of sun-spots and magnetic variations.

The differences of pressure shown by the isobars passing through the three stations was least in 1844, and greatest in 1847-48*. It does not follow, however, that the tenth-inch isobars were really further separate in 1844 than in 1847-48, since this depends also on the perpendicular distance between the isobars, or on the angle θ which they make with the meridian; thus we find the perpendicular distances of the tenth-inch isobars in geographical miles in the different years to be as follows:—

1842.	1843.	1844.	1845.	1846.	1847.	1848.	1849.
218	273	317	242	273	266	281	330

Thus the isobars were furthest separate in 1844 and 1849, and nearest in 1842 and 1845. The regular variation of β_1 and β_2 was thus chiefly due to the inclination of the isobars to the meridian. From the last line of Table IV. we see that the isobars were from 10° north of west (W. by N. nearly) in 1844, and from 19° south of west (W.S.W. nearly) in 1848. Though there is a coincidence in these epochs with those of minimum and maximum sun-spot frequency, it is a coincidence which requires confirmation by a longer series of observations, the more especially as the change of value of θ from 1848 to 1849 is very considerable.

Annual Variations.—The following Table contains the monthly mean values for the whole eight years, those of θ being derived from the mean values of β_1 and β_2 by equation (4) (they are therefore not the means of the quantities in Table IV.). β_1 and β_2 , with their probable errors, are in thousandths of an inch.

TABLE V.—Monthly Means from eight years' Observations.

Month.	Mean. Green- wich, +29.	β_1 .	Pro- bable error of β_1 .	β_2 .	Pro- bable error of β_2 .	θ .	Pro- bable error of θ .	$\beta = 0.1$ in.
	in.							miles.
January.....	0.949	+163	± 20	92	± 14	261°	± 9	157
February	0.921	+133	26	54	19	271	13	186
March	0.923	+ 96	15	42	15	271	12	254
April	0.880	+ 28	22	11	15	275	25	839
May	0.964	+ 12	21	14	20	216	25	1710
June	0.966	+ 85	8	36	8	272	5	285
July	0.978	+110	9	46	7	273	3	220
August	0.958	+ 93	9	29	9	280	5	220
September	1.004	+ 33	17	1	9	295	15	546
October	0.870	+110	11	55	12	267	7	238
November	0.905	+138	12	100	9	249	7	186
December	1.026	+133	31	128	23	230	13	178
Year	0.945	+ 95	4	+50	4	264	2	267

* This result was obtained for β_1 from the Greenwich and Makerstoun Observations in 1850 (Trans. Roy. Soc. Edinb. xix, part 2, p. xci).

Annual Variation of Barometric Pressure in England.—In the discussion of the Makerstoun observations of barometric height for the years 1842–49 I sought to determine this law, and found that the greatest pressures occurred in the months from May to September (both inclusive), while the least occurred in October and November; the probable errors of the monthly means were, however, found to be too considerable to give much value to the secondary variations which were shown in the monthly mean values. The mean for the six months April to September was one tenth of an inch greater than that for the six winter months*. Very similar conclusions were arrived at by Dr. Lloyd, in his discussion of the Dublin observations for 1840 to 1850, both as regards the epochs of maximum and minimum pressure, as well as of the probable error (or divergence) of the monthly means†. Somewhat similar conclusions may be deduced for Greenwich from the second column of Table V., the maximum in December being slightly more marked than at Makerstoun and Dublin.

If the isobars made the same angle with the meridian in all the months of the year, and kept at the same interval, the annual variations would be the same at all the stations. This, however, it will be seen, is not the case, and the annual variation of barometric pressure for any place is a compound result.

I have sought to determine the annual law for the centre of gravity of the three stations (a point a little to the east of Liverpool) as derived from the monthly means, reduced to the sea-level, for the three places; these are given below.

	in.	Probable error. in.		in.	Probable error. in.
January ..	29·864	0·027	July	29·926	0·015
February ..	·859	·049	August	·917	·019
March	·877	·035	September ..	·993	·022
April	·867	·039	October	·815	·033
May	·956	·027	November ..	·826	·027
June	·926	·022	December ..	·939	·049

The probable errors are least for the four months of June to September, and greatest in the months of December and February. The probable errors are much diminished if we determine the means for groups of months in each year. Thus we find :—

	in.
October and November	29·820 ± ·016
December to April	·881 ± ·017
May to August	·931 ± ·010
September	·993 ± ·022

* Trans. Roy. Soc. Edinb. xix. part 2. p. xci, 1850.

† Observations made at the Magnetical and Meteorological Observatory at Trinity College, Dublin, under the direction of Humphrey Lloyd, D.D., D.C.L., vol. ii. p. 347, 1869.

The probable errors are here sufficiently small, when compared with the variations from group to group, to allow us to conclude that we have obtained a considerable approximation to the law of change for the centre of England. It is not a little remarkable that the greatest monthly mean pressure (that for September) is followed immediately by the least, the probable errors leaving no doubt as to this fact*, the difference of pressure from September to October–November being 0.173 inch. It is not improbable that the pressure in December is greater than in the months immediately following; but a longer series of observations is required to determine that question, since the probable error for the month is very large.

The mean barometric pressure at the mean sea-level for a point in 53° 24' N. and 2° 48' W. of Greenwich is

$$29.897 \text{ inch} \pm .010,$$

as deduced from the eight years' observations (1842–49) at the three stations.

Annual Variation of the quantities β_1 , β_2 , and θ .—It will be seen from Table V. that the magnitude of β_1 and β_2 is a maximum in December and January, a minimum in April or May, a secondary maximum in July, and a minimum again in September. This result was obtained by me for β_1 in 1850. The perpendicular distances between the isobars calculated with the aid of the values of θ are given in the last column of Table V.: the distances are least about January and about July; they are greatest in April, May, and September. The following are the differences of barometric pressure corresponding to 100 geographical miles perpendicular to the isobar.

* The probable errors given above and in Table V. have been obtained in the same way as for the observations of a constant quantity. There can be no doubt, however, that the conditions are not strictly the same in the two cases. In the case of deviations of observations of magnetic declination from the mean, I have shown, in the *Makerstoun Observations for 1844* (*Trans. Roy. Soc. Edinb.* vol. xviii. p. 351), that they did not satisfy the hypothesis of positive and negative distribution employed in the calculus of probabilities for the probable error. This is also true for various meteorological phenomena. A consideration of the differences of the monthly mean barometric height has induced me to believe that the formula for the probable error gives such an approximation to the probable deviation of any determination that it may be accepted as a relative measure of the exactness of the result. It will be obvious also that these differences cannot be called errors if they are due to a law. In the present instance it has been remarked that β_1 , β_2 , and θ seem all to have a somewhat regular variation depending on the year. It will be seen, however, from the last line of Table V., that if we assume this variation to be accidental, the probable difference due to it is so small that it can only slightly increase the probable differences or errors for the different months.

	in.		in.
January.....	0.064	July	0.045
February	0.054	August	0.045
March	0.039	September	0.018
April.....	0.012	October.....	0.042
May	0.006	November.....	0.054
June	0.035	December	0.056
The year	0.038 inch.		

It thus appears that the difference of pressure for 100 miles is greatest in the coldest months, is a secondary maximum in the warmest months, and is a minimum in April, May, and September*. The only laws resembling this are those for magnetic disturbance, frequency of the aurora borealis, and for the horizontal force of the earth's magnetism.

Annual Variation of the direction of the Isobars.—The values of θ given in Table IV. can be considered as only rough approximations in some months. The probable errors of the mean result (Table V.) have been calculated from Table IV., giving each determination equal weight. I have found that the probable errors thus obtained differ little from the mean deviation of θ , deduced from equation (4) with the limiting values of β_1 ($\pm$ p. e.) and of β_2 ($\pm$ p. e.), excepting for the months of May and April, for which the mean deviations are much greater. This is due to the very small *mean* values of β_1 and β_2 , and their comparatively large probable errors for these months.

From the seventh column of Table V. it appears that the isobars run most from south of west in December (nearly from S.W.) and in May (S.W. by S.), and most from north of west in September. Beginning with October, the isobars run from a little south of west; they are from further south in November, and from furthest south of west in December. They again approach the west point in January, remain nearly west and east (within a few degrees) from February to July (excepting May, to which I shall refer presently); move further north in August, to the maximum north of west (25°) in September.

The lines from May run from 54° south of west (to 54° north of east); but this result cannot receive much value, as the probable errors are greater than the mean values of β_1 and β_2 . This remark applies also, to a great extent, to the result for April, which, however, agrees nearly with

* I think there is some reason to believe that this law, as regards the months, with some slight modification, is general. Mr. Buchan, in his valuable "first approximation" to charts of isobars for the whole earth, has stated that April is the month in which "pressure is more equally distributed over the globe than in any other month" (Edinb. Trans. vol. xxv. p. 578, 1869). This conclusion depends on the value of the approximation to the isobars for the southern hemisphere, for which the data are not only least numerous, but probably least exact. Mr. Buchan's charts show May to be the month of widest distribution for the northern hemisphere, as has been found here for England.

the directions for February and March. I shall return to the results for April and May in the next part of this paper.

The directions here obtained from eight years' observations, made under the most unexceptionable circumstances in three observatories, will probably not be improved for some time*. On this account I have thought it desirable to project the isobaric lines on small charts, so as to show at a glance their general direction and distribution (see Plate 12). The results for the whole year have so small probable errors that I have projected them on a larger scale† (see Plate 11).

It should be here remarked that there is no ground as yet for affirming that the mean direction and distribution of the isobars for any series of years will be the same as for any following series; and the best determination that can be obtained for any given time may be of the greatest importance for comparison with another of equal value at a later period for the answer to this question.

2. *Relation of the Direction and Interval of the Isobars to the Direction and Force of the Wind.*

These relations have been hitherto studied chiefly with reference to cases of strong winds or storms. The conclusions have been obtained directly from the projections on charts of the isobaric lines and wind-directions on particular days. In the following part of this paper, as in the preceding, the results are sought from the observations made on every day throughout the series of years employed; they may therefore serve as a base for the study of particular cases.

* I have sought to obtain some confirmation for the directions of the isobars in Great Britain from the numerous observations made in 1857 to 1867, employed by Mr. Buchan in the formation of his charts. Mr. Buchan's object in the construction of these charts appears to have been to obtain the best possible approximation to the broad features of the distribution of the isobars for the whole earth; and he has exercised his judgment in employing the mass of observations which he was able to collect (Edinb. Trans. vol. xxv. pp. 575, 576). My examination of these for the months of April and May, in which the mean change of pressure for a distance of 100 miles is only a few thousandths of an inch of mercury, has shown the difficulty of deducing from them any accurate determination, such as is here attempted, for a small space like England.

† These charts are not projections, and were devised by me for another investigation, for which the true areas on the sphere were required; they are, however, probably not new. The following will give the most simple idea of their construction. If we suppose the surface of a globe to be built up of series of rings of cylindrical wires laid on the parallels of latitude, and cut out the wires covering any part of the surface, then if these be laid out touching each other on a plane surface, in straight lines perpendicular to a central or principal meridian, the external meridians will be curves of sines (straight lines for 20° squares on the scale of the chart for the year). The distances of any point perpendicularly to a circle of latitude and to the principal meridian are the true lengths of the arcs in latitude and longitude from these circles. For small spaces the distortion is inconsiderable. In the charts here given the principal meridian, that of Greenwich, is not in the middle, the direction of the isobaric lines having been referred to that meridian.

Directions and Force of the Wind.—In most meteorological investigations, and especially in those connected with climate, it is essential to obtain the force and frequency of the winds for each point of the compass. It has been usual, however, in many researches to determine the *resultant* direction and force for given periods by the known law of the composition of forces. It is quite possible that this resultant may be in a direction from which no wind ever blows, but it does not the less represent what all the other winds would have been equivalent to, both in direction and force. It may also be affirmed that in fact the resultant direction thus obtained is generally that of the prevailing wind.

In questions such as the present, if any general relation subsists between the direction of the isobars and that of the winds, the mean direction of the one will probably be affected in like manner with that of the other*. It occurred to me then to compare the *resultant* directions of the wind with those of the isobars for each month of the year. Possessing these resultants for Makerstoun for each month derived from four years' observations (1843–1846) †, it was necessary to compute the corresponding values for Greenwich and Dublin, and to calculate the mean directions and intervals of the isobars for the same years.

The observations at Greenwich were made with an Osler's anemometer, and only pressures which on the mean for an hour exceeded one fourth of a pound were noted. At Makerstoun the instrument employed was of a wholly different description and not self-registering; observations were made of the maximum pressures (if not less than one tenth of a pound) within ten minutes at each observation hour (see the Introductions to the Makerstoun Observations). Observations of the direction and force of the wind were not begun at the observatory of Trinity College, Dublin, till 1845; but I have obtained observations for the four years in question from the volume of observations made at the Ordnance Observatory, Phoenix Park, in a locality stated by Dr. Lloyd to be wholly unexceptionable‡. The instrument employed at the Phoenix Park observatory was a Whewell's anemometer, which always worked imperfectly, so that the observed runs of the pencil differed from time to time for the same wind-velocities §. The monthly resultants calculated from the

* It is to be observed that the mode in which the resultant wind-direction is obtained is not the same as that employed for the direction of the lines of equal pressure; but this difference can only affect results derived from periods during which the wind has blown frequently in very different or opposite directions; and even in such cases the isobars should retain their relation to the predominating wind, if the resultant force for that direction is considerable. A special investigation will show, however, the amount of error which may be due to this cause.

† Trans. Roy. Soc. Edinb. vol. xix. pt. 2, p. 99.

‡ Observations made at the Observatory, Trinity College, Dublin, vol. i. p. 384.

§ Introduction to Observations made at the Phoenix Park Observatory.

numbers of runs have been combined by me in the same way as for the forces to obtain the means for the four years.

The resultant forces cannot therefore be considered absolute measures at any of the three stations; I have, however, endeavoured to reduce them to a common unit*.

In order to determine the factor required to give approximately the true wind-pressures, I have assumed that the mean velocities of the wind at Greenwich and Makerstoun do not differ much from those shown by Robinson's anemometers at Kew and Oxford. Mr. Johnson found that the mean velocities at Kew and Oxford were nearly the same †; the mean velocity at Oxford, 110 feet above the ground, from four years' observations (1857-61) was 10·2 miles an hour. This velocity, employing the generally accepted formula $P = \frac{V^2}{200}$, gives a constant mean pressure of 0·52 lb. on the square foot of surface ‡; the mean of the observed pressures during the four years 1843 to 1846 (without reference to direction) was 0·46 lb. at Makerstoun and 0·48 lb. at Greenwich, an agreement which, considering all the circumstances, the difference of instruments, modes of observation, and distances from the ground of the air stratum whose force was measured, must be quite accidental.

The resultant forces for Makerstoun have then been multiplied by 1·13 and those for Greenwich by 1·08 to reduce to the mean, 0·52 lb. at Oxford. In the case of the pencil-runs on Whewell's anemometer at Dublin they have been divided by 50, which gives an approximation to the mean pressures at the other stations. The means of the three resultant pressures found for each month from the four years' observations may then be taken as approximations to the resultant mean pressure for the mean resultant direction over the space included by the three stations.

The following Table contains the mean barometric pressure at Greenwich, with the differences (β_1 and β_2) from the mean pressures at Makerstoun and Dublin (at mean sea-level), and the resultant directions of the wind at the three stations.

* At Dublin and Makerstoun the vanes were nearly 20 feet above the ground (at Greenwich I believe the height was greater). I do not know any objection to the position of the vanes which could vitiate the observed *directions* of the wind at any of the stations. From my knowledge of the instrument used at Makerstoun and of the care with which the observations were made, I believe the means to be very good *relative* measures of the forces.

† Radcliffe Observatory Observations, vol. xviii., introduction to Meteorological Observations in 1857, p. xxiv.

‡ This formula is, I believe, very far from being accurate; the true relation between the pressure and velocity of the wind, as shown by different anemometers, requires an integrating pressure instrument, in which the sums of pressures may be shown by work done.

TABLE VI.—Directions of the Isobars and of the Wind, 1843–1846.

Month.	Green- wich barom. +29 in.	β_1 .	β_2 .	θ .	Resultant wind-direction.				$\theta - \phi$.
					Mak.	Dubl.	Green.	Mean ϕ .	
January ...	in. 0.912	in. +1.85	in. +0.91	267°	241°	255°	246°	247°	+20°
February ...	0.843	+0.68	+0.05	293	287	298	256	280	+13
March	0.907	+0.84	+0.42	267	244	228	245	239	+28
April	0.918	+0.64	+0.37	260	274	266	241	260	0
May	0.948	-0.26	-0.40	22	24	-27	3	0	+22
June	0.959	+0.91	+0.51	262	233	255	230	240	+22
July	0.946	+1.06	+0.43	274	230	276	241	249	+25
August	0.920	+0.86	+0.17	287	247	300	246	264	+23
September	1.052	+0.37	+0.09	284	244	261	224	243	+41
October ...	0.805	+1.43	+0.71	267	237	267	232	245	+22
November	0.876	+1.36	+1.12	240	211	251	216	226	+14
December	1.051	+1.04	+0.67	255	239	270	262	257	-2
Year	0.928	+0.90	+0.42	269	243	261	243	249	+20

Relation of the Direction of the Isobars to that of the Wind.—The resultant direction of the wind at Greenwich is the same as that at Makerstoun on the mean of four years, but the monthly directions differ on the average 12° (without reference to sign). The resultant direction at Dublin for the four years differs considerably (18°) from that at the other two stations. The mean of the three resultant directions (ϕ) is given in Table VI., together with the differences ($\theta - \phi$).

It appears that the direction of the isobars was *positive* of that of the wind in ten months of the year, the directions being nearly the same in two months, April and December; the difference was greatest in September. From the mean directions for the four years we find

$$\theta - \phi = +20^\circ *.$$

I believe that the only really exceptional result is that for December, since the determination of θ for April and September depends on very small values of β_1 and β_2 ; the mean of the results for these two months is, however, exactly the mean for the year. The most remarkable of the

* "The wind in storms neither blows round the centre of least pressure in circles, or as tangents to the concentric isobars, nor does it blow directly towards that centre. It takes a direction intermediate, approaching, however, more nearly to the direction and course of the circular curves than of the radii to the centre" ("On the Mean Pressure of the Atmosphere," by Mr. A. Buchan, Edinb. Trans. vol. xxv. p. 581); that is, the angle with the isobar is less than 45° . Mr. Buchan adds, "or, according to Dr. Buys-Ballot, the angle is not a right angle, but from 60° to 80° ;" that is, from 30° to 10° with the isobar. The mean of these limits is 20° , exactly what has been found above from the mean direction of the isobars and resultant direction of the wind in all cases. I have not been able to find any memoir by the distinguished Utrecht meteorologist containing the grounds on which his limits are founded. The tendency of the winds inwards (towards the centre) in cyclones is noticed by several early writers on the subject, among others by Redfield (Silliman's Journal, vol. i. p. 14, 1846).

differences is that for May, which also depends on small negative values of β_1 and β_2 , yet gives a result differing but little from the mean, though the direction of the wind (a little to west of north) is upwards of 100° from the mean direction*.

Relation of the difference of barometric pressures for 100 miles to the resultant force of the wind.—The differences of barometric pressure for 100 geographical miles perpendicular to the isobars (100 mile *gradients* †) have been calculated from the values of β_1 , β_2 , and θ ; these, with the resultant mean pressures for the wind obtained as indicated p. 525, are given in Table VII. The 6th column contains the mean pressure (P) of the wind in pounds on the square foot of surface, derived from the results for the three stations. The last column contains the differences of barometric pressure for 64 miles of interval, for which, in the mean, one thousandth of an inch of barometric pressure is equivalent to one hundredth of a pound of pressure of the wind.

TABLE VII.—Resultant forces of the Wind and difference of Barometric Pressure, 1843–1846.

Month.	β for 100 miles	Resultant force of wind.				β , for 64 miles.
		Green.	Mak.	Dubl.	Mean P.	
	in.	lb.	lb.	lb.	lb.	in.
January	0·074	0·66	0·49	0·71	0·62	0·047
February	·035	·22	·20	·14	·19	·022
March	·035	·21	·27	·28	·25	·022
April	·028	·34	·11	·20	·22	·018
May	·016	·23	·07	·09	·13	·010
June	·036	·27	·25	·17	·23	·023
July	·046	·29	·23	·20	·24	·028
August	·040	·25	·12	·14	·17	·026
September	·017	·08	·08	·04	·07	·011
October	·057	·41	·26	·32	·33	·036
November	·054	·40	·29	·23	·31	·035
December	·040	·16	·37	·26	·26	·026
Years	0·036	0·25	0·20	0·23	0·23	0·023

* This agreement is probably to some extent accidental: it will be seen that the direction of the wind at Dublin was -27° (27° west of north), while it was near north at Greenwich, and 24° east of north at Makerstoun. When the resultant direction for May at Dublin is determined by the frequency of the wind from different points (by Lambert's formula), it is found to be $+20^\circ$, nearly as at Makerstoun; the difference, $\theta - \phi$, for May would then become $+6^\circ$. I may remark that the resultant directions for the other months at Dublin obtained by the same formula agree better with the mean of those at Greenwich and Makerstoun than when the "runs" of the pencil are employed. The resultant direction for the four years is 256° by the frequency, or 5° further south of west than when determined by the pencil-runs. The resultant direction at Makerstoun for the four years is exactly the same by the two methods.

† I have felt some difficulty in employing the word gradient, due to Mr. Thomas Stevenson, C.E., in this relation, as it is associated to some extent with a hypothesis of

It has already been stated that the resultant pressures of the wind are only rough approximations, yet these indicate very distinctly the same annual law of variation as the *diabars*; not only so, the results for each month show no great divergences from the ratio stated above derived from the mean for the four years.

When we remember that these resultant forces are obtained in some cases from winds proceeding from opposite directions, which destroy each other to a great extent, and that the mean differences of barometric pressure are obtained similarly from pressures which increase in opposite directions, it seems probable that the relation found always holds, though the ratio may vary somewhat with winds of different forces and from different directions. The variations including the greater divergences from the mean must also be the object of special investigations, in which winds from the same direction, and with nearly the same mean forces, must be considered alone.

Ratio of the mean pressure of the wind (surface current), independent of direction, to the resultant pressure.—It has been stated that the pressure of the wind was always noted at Makerstoun whenever the maximum pressure within an interval of 10 minutes at the hour of observation was at least one tenth of a pound on a square foot of surface, and that although the absolute mean pressures cannot be derived directly from these observations, yet that their *relative* values are probably determined with considerable accuracy. We may then obtain a measure of the variability of the wind, that is of the degree of opposedness of the masses of air in motion at the earth's surface, from the ratios of the mean (M) to the resultant (R) pressures. Those at Makerstoun for the four years, 1843 to 1846, were as follow * :—

	M. lb.	M R	I. miles.		M. lb.	M R	I. miles.
January	..0.72	1.5	135	July	0.40	1.7	227
February	..0.63	3.1	286	August	...0.32	2.5	250
March	...0.63	2.3	286	September	.0.27	3.4	588
April	...0.52	4.6	357	October	..0.58	2.2	175
May	...0.51	7.5	625	November	..0.59	2.0	185
June	...0.51	2.0	278	December	..0.60	1.6	250

We see that the greatest proportion of the surface current proceeded from one quadrant in the winter months of December and January, and in the summer months of June and July; that the wind was most variable in May, April, and September. We have already found (p. 522)

atmospheric surfaces with hills and valleys, into the latter of which the former are supposed to flow. On this account I should much prefer some other word (such as *diabar* or *barode*), which would serve to indicate the relation of the difference of pressure to the unit of distance.

* "Results of Makerstoun Observations," Trans. Roy. Soc. Edinb. vol. xix. pt. 2, art. 209, p. xvi (multiplied by 1.13, as before).

that the directions of the isobars varied most in the same months, May being the month of greatest variability*.

I have added the perpendicular distances (I) between the tenth-inch isobars in geographical miles (deduced from the values of β , second column of Table VII.). These intervals show a remarkable constancy of ratio to the variability of the wind. In the mean for the four years at Makerstoun,

$$\frac{M}{R} = \frac{0.52}{0.20} = 2.60; \text{ I} = 278 \text{ miles.}$$

The intervals between the tenth-inch isobars increase with the variability of the wind. These, it will be remembered, are mean results. The mean pressure of the wind has its mean value in the two months of April and May, when the direction is most variable; but in September, the next epoch of maximum variability, the mean pressure is a minimum.

It appears also from the fourth column of Table VIII. that the direction of the cirrus current was most variable in April and May; the next epoch of maximum variability was, however, in August instead of September, as for the surface current.

3. *Atmospheric Currents.*

In the preceding investigations we have employed the observations of the direction of the surface current only. We require greatly to know to what extent the direction would vary in ascending from the earth. The late Mr. Johnson, Director of the Oxford Observatory, found that an anemometer at a height of 110 feet from the ground showed a wind-velocity two and a quarter times that indicated by a similar instrument 22 feet from the soil; part of this difference was probably due to buildings and trees in the neighbourhood; but there can be little doubt that proximity to the soil is a cause of diminished velocity. I am acquainted with no observations, made at points free from all obstructions, which can answer the question, whether the *direction* of the wind varies considerably within a few hundred feet of the ground. The only way, then, of investigating this subject must be by comparisons of the direction of the wind near the surface, in well-exposed localities, with that of the motions of the clouds.

Such observations require in general much time; to be of any use to science they should be made frequently and systematically. This can be

* I may remark here that the variability of the direction of the wind and of the intervals and direction of the isobars have no relation whatever to the diurnal variation of temperature, nor to the variation of temperature from day to day. The diurnal range of temperature was greatest in the mean of the four years in June and August. The difference of the daily mean temperature from the monthly mean was least in August, May, and July; less in these months than half the mean difference for January (5°). ("Results of Makerstoun Observations," Trans. Roy. Soc. Edinb. vol. xix. pt. 2, p. lxxxv.)

done well only at observatories where assistants are continually on the watch, during the day at least; the employment of self-registering instruments has diminished the number of observers required in permanent institutions, and observations of cloud-motions can now scarcely be expected with the requisite completeness. On board ship these observations are made with even more difficulty than on shore; rapidly moving upper currents, especially those in which clouds are seen moving from very different or nearly opposite directions, catch the eye at once, and are most frequently recorded. No serious investigations can be founded on observations when the most frequent motions are really not observed.

Believing that any just conclusions as to the atmospheric currents could be obtained only from long and careful observations of cloud-motion, I began a series of observations in 1842 at Makerstoun, which I continued in the following years with my assistants, the late Mr. John Welsh, F.R.S., and Mr. A. Hogg*. Four currents were distinguished at different heights. 1st, the surface current observed by the wind-vane; 2nd, the current of loose cumulus (*scud*) and cumulus; 3rd, the current of cirro-stratus; 4th, the current of cirro-cumulus and cirrus†. The observations of these currents have been only partially discussed; I shall give here some of the results which bear upon the subject of the present investigation, derived from the observations made in the four years 1843 to 1846, for which the directions of the surface-wind and of the isobaric lines have been already obtained.

Direction of the Cirrus current.—Having found the *mean* direction in which the cirri and cirro-cumuli moved for each day on which their motion was observed during the four years, I have combined these by Lambert's formula (on the assumption that the velocity was the same on each day) in order to obtain the resultant direction (ψ) for each month; these are given in Table VIII.

* The motions of the clouds were determined by seeking till a marked portion was found which seemed to run up or down one of the four corners of the observatory, the points of the compass relatively to which were marked on a surrounding paling; when no portion of cloud could be found that would pass or had passed through the zenith, the vanishing-point of the motions of different portions of cloud in the same stratum could generally be ascertained very nearly. It is believed that in a great majority of cases the directions were found to within half a point of the compass, though the nearest point was always noted. In the year 1844 alone 4370 observations were made of the directions of motion of the cloud-currents.

† The same classification of cloud-motions was proposed by M. Poey to the Paris Academy of Science in 1864 (*Comptes Rendus*, t. lviii. p. 669), excepting that he separated the loose cumuli from the cumuli: this separation I had also made, especially in the north-east quadrant, in which the loose cumuli have a much lower position than in the other quadrants; but in taking the mean directions the two strata were included in one. M. Poey has, I believe, recommended this system of observation of the atmospheric currents to the Meteorological Committee of the Royal Society, a recommendation in which I entirely concur.

TABLE VIII.—Resultant Direction of Cirri and Surface-wind,
1843–1846.

Month and Year.	No. of days.	Cirri resultant.		Wind res. direc. ϕ .	$\psi - \phi$.
		Dir. ψ .	No. p. 100.		
January	57	279 ⁰	62.6	241 ⁰	+38 ⁰
February	41	303	62.7	287	+16
March	42	301	62.7	244	+57
April	50	242	39.4	274	-32
May	44	291	39.9	24	-94
June	51	263	56.9	233	+30
July	42	269	58.0	230	+39
August	56	267	48.0	247	+20
September	34	274	60.4	244	+30
October	35	276	56.0	237	+39
November	38	236	56.9	211	+25
December.....	44	308	69.3	249	+59
1843	119	276	48.0	249	+27
1844	155	288	56.4	249	+39
1845	150	280	50.9	247	+33
1846	110	259	59.1	229	+30
4 years.....	534	277	52.0	243	+34

I have entered for comparison the resultant directions (ϕ) of the surface current at Makerstoun. It will be seen that in ten months of the year the values of $\psi - \phi$ are positive; in two months, April and May, they are negative*: the greatest positive difference is that for December.

When we determine the resultant directions from all the observations in each of the four years (see Table VIII.), we find that the difference of directions of the cirrus and surface currents may be given as $34^{\circ} \pm 2^{\circ}$.

It has to be pointed out, however, that these directions are not strictly comparable; the direction of the wind is observed at all hours when the wind blows; those of the currents carrying clouds can be observed only when clouds exist in them. The motion of the cirri was observed at Makerstoun only in two days out of five, whereas the direction of the wind was observed on most days in each year. This fact may affect the exactness of the differences for limited periods; but it will be shown that it affects little the final results. There is, however, a correction required that should not be neglected. The observations of cloud-motions were generally during the day, that is, on the mean of the year, from

* This difference is, I believe, chiefly due to the north-east winds which blow so frequently in these months. The scud carried by this current is the lowest cloud observed. This and the cirrus stratum appear generally to be set in motion by different causes; the cirri move rarely from an easterly point, and the maximum of frequency shown for north-east winds in Great Britain does not appear in the results for the cirrus stratum (*Comptes Rendus*, t. lxxxi. p. 34, 1875).

6 A.M. to 6 P.M. Now at Makerstoun during the four years 1843–1846 the mean of the resultant directions for the hours 6 A.M. to 6 P.M. was 248° . Hence we have

$$\psi - \phi = 277^{\circ} - 248^{\circ} = +29^{\circ}.$$

So many causes may affect the direction of the surface current in certain places that it cannot fail to appear possible, if not probable, that we have here a result depending on local conditions, contour of the country, or accidental causes. This possibility it is essential to consider.

In the first place it will have been remarked that the mean direction of the wind from the four years' observations, 27° south of west, is the same as has been deduced from the Greenwich observations. In the next place, the mean direction of the wind for Scotland, deduced from eleven years' observations (1857–1867) at 9 A.M. and 9 P.M., made at *fifty-five* stations, was 32° south of west*, while at Makerstoun, from observations at the same hours in the four years, 1843 to 1846, it was 29° south of west†—a difference of only 3° , which would go to increase that of $\psi - \phi$.

Fortunately I have been able to confirm the result for the difference of directions of the cloud and surface currents by observations made in a very different locality. M. Quetelet observed the directions of the cloud-motions (without distinction of species) at the Brussels Observatory during the years 1833 to 1846, and he found the resultant directions by Lambert's formula as follows ‡:—

In the 7 years 1833 to 1839....(ψ)= $258^{\circ} 53'$

Do. 1840 to 1846.... „ = $255^{\circ} 21'$

In the 14 years 1833 to 1846.... „ = $257^{\circ} 50'$;

whereas by the wind-vane of Osler's anemometer (which was erected in 1842) the direction was found

In the 5 years 1842 to 1846.... ϕ = $225^{\circ} 49'$

A comparison of the numbers of times the clouds and surface-wind had moved from each point of the compass in the five years 1842 to 1846 induced Quetelet to think that the clouds, as well as the resultant surface-wind, had really proceeded from a more southerly point than in the preceding years. Had the eminent director of the Brussels Observatory sought the resultant directions for each year, he would have found that marked differences of the direction of motion existed in each of the five years. I have calculated these directions from Quetelet's numbers; they are as follows:—

* This result I have deduced from Table II. of Mr. Buchan's paper already cited, Trans. Roy. Soc. Edinb. vol. xxv. p. 616.

† "Results of Makerstoun Observations," Trans. Roy. Soc. Edinb. vol. xix. pt. 2, p. c.

‡ Sur le Climat de la Belgique, 2^{de} partie, p. 8.

Year.	(ψ).	ϕ .	(ψ)— ϕ .
1842.....	230 ⁰	221 ⁰	+ 9*
1843.....	257	232	+ 25
1844.....	283	251	+ 32
1845.....	251	219	+ 32
1846.....	252	216	+ 36
5 years.....	255	226	+ 29

If we admit that a correction should be applied, as at Makerstoun, on account of the difference of the direction of the wind during the day hours when the cloud-motions are observed, we find for Brussels

$$(\psi) - \phi = 24^{\circ}.$$

The agreement of the results for the last four years, which are the same as in the preceding discussion, is very remarkable; thus the direction of the surface-wind at Brussels became 32° more southerly in 1845 than in 1844, and the direction of the cloud-motion changed exactly by the same number of degrees and in the same direction. A similar though less marked result will be seen at Makerstoun (Table VIII.), the cirri moving from a point 21° more southerly in 1846 than in 1845, while the surface current changed 18° in the same direction.

It has been remarked (p. 531) that the wind and cloud directions are not exactly comparable, as the latter cannot be observed so frequently as the former; this fact has less weight in the results for Brussels than in those for Makerstoun, since the former include clouds of all kinds, which are observed much oftener than the cirri alone. There is, however, another method of comparing the directions of motion which was employed by me at first in the discussion of the Makerstoun observations for 1843. The *differences* of the directions of the surface and cloud currents when observed simultaneously were noted: when several such comparisons made at successive hours gave nearly the same difference, the mean was termed a "result;" when the differences varied considerably two or three results might be obtained in the same day. From these results for the four years 1843 to 1846 the following mean differences were obtained:—

1. Cirrus current minus surface current.

Total.	Number of results. Per 1000.			Mean difference.
	+	—	=	
359	769	181	50	+ $29^{\circ}6$

The total number of results was 359; as the motions of the cirri were observed on 534 days in the four years, there were several days on which no surface current was blowing when the motions of the cirri were

* The small value of (ψ)— ϕ for 1842, compared with those for the following years, may perhaps be due to some instrumental cause, as this was the first year in which the anemometer was employed.

determined. The numbers of results in 1000 for which $\psi - \phi$ was positive (+), negative (-), or the same (=) are given. The final value of $\psi - \phi$ is the same to a fraction of a degree as that found, p. 532. This confirmation is of importance, as it shows that the method of calculating the resultant directions does not affect the accuracy of the final values.

It is probable that this difference between the directions of the cirrus and surface currents will not take place *per saltem*, and that the observations of cloud-motions at different heights should show some variation in the value of $\psi - \phi$. If we should find that the lowest stratum of clouds, that of scud and cumuli, which may be considered generally at from 2000 to 5000 feet above the ground, has its direction intermediate between that of the cirrus and that of the surface, we shall have a most conclusive proof, were other proof needed, that the differences of motion found are not due to local causes.

In the four years 1843 to 1846, 339 results were obtained from simultaneous observations of the cumulus and cirrus motions; from these and comparisons of the other currents the following quantities are derived, where ψ , ψ' , ψ'' , and ϕ are the mean directions of the cirrus, cirro-stratus, cumulus, and surface currents respectively:—

2. *Cirrus current minus scud and cumulus current.*

Total.	Number of results. Per 1000.			Mean difference.
	+	-	=	
339	666	251	83	$\psi - \psi'$ $+ 13^{\circ} 7$

The difference is somewhat less than half of that between the cirrus and surface currents. As the currents approach each other the differences become smaller, and the relative number of negative results increases. Thus when we examine the 683 results derived from simultaneous observations of the cirro-stratus and cumulus currents which pass into each other, we find:—

3. *Cirro-stratus current minus cumulus current.*

Total.	Number of results. Per 1000.			Mean difference.
	+	-	=	
683	568	297	135	$\psi' - \psi''$ $+ 6^{\circ} 9$

which is again nearly half the difference between the cirrus and cumulus current.

Having found, I think conclusively, that the differences of motion observed between the surface and cirrus currents are independent of circumstances of locality, we may with confidence examine the results derived from the more numerous comparisons of the surface current with the cirro-stratus and cumulus currents. Thus we find:—

4. *Cirro-stratus current minus surface current.*

Number of results.				Mean difference.
Total.	Per 1000.			
	+	-	=	$\psi' - \phi$.
754	788	183	29	+ 22°.8

5. *Cumulus current minus surface current.*

Number of results.				Mean difference.
Total.	Per 1000.			
	+	-	=	$\psi''-\phi$.
1434	791	171	38	+14°·5

If we subtract the 5th mean difference from the 1st and from the 4th, we find

$$\begin{aligned}\psi - \psi'' &= + 15^{\circ} \cdot 1, \\ \psi' - \psi'' &= + 8^{\circ} \cdot 3.\end{aligned}$$

These two mean differences are slightly greater than were found by the direct comparisons 2 and 3.

If we give the 1st, 4th, and 5th mean differences weights corresponding to the number of results from which they are obtained, then we find

$$(\psi) - \phi = + 19^{\circ} \cdot 1 ;$$

and if the three mean differences receive equal weights,

$$(\psi) - \phi = + 22^{\circ} \cdot 3 ;$$

where (ψ) indicates, as before, the mean direction from the north (through east) of *all* the species of clouds. The former of these results may represent approximately the *observed* difference when no attention has been paid to the species of cloud : the latter may be considered an approximation to the mean difference for the whole mass of air in motion. Each of these results is somewhat less than that found for Brussels (24°).

We have no knowledge of the mean law according to which the velocity of the air in motion varies with height above the ground. From previous observations, while observing the aerial currents at Makerstoun, I had concluded that the maximum velocity occurred rarely at a greater height than 6000 feet, and that the *mean* stratum of maximum velocity was probably not above 5000 feet. From this height, then, the velocity diminishes as we ascend and descend, but according to different laws. At the *surface* of level ground the velocity is, on an average, probably not one third of that at the maximum ; the upper limit, where the motion ceases, is in all probability little above the average height of the cirri. I had frequently observed at Makerstoun that the angular movement of cirri passing through the zenith was generally very much smaller compared with that of the lower clouds than could be explained by the difference of heights had the velocities been the same.

If we remember the increased dimensions of a unit of mass of the atmosphere as we ascend and take the equation

$$mv_h = \frac{1}{n} \Sigma(mv),$$

where m is the unit of mass, v_h the velocity at a height h , n the number of units in the vertical column in motion, and $\Sigma(mv)$ the sum of the units of mass in the column into their velocities, then it seems probable that the height h will not be higher than the stratum of the cirro-stratus, nor lower than that of the scud and cumulus. In all probability, then, we shall have $(\psi) - \phi$ between 23° and 15° ; and there cannot be any considerable error in concluding that for the height h

$$(\psi) - \phi = +20 \text{ approximately}^*.$$

It will be remembered that we have already found (p. 526) that

$$\theta - \phi = +20^\circ;$$

and since ϕ , the direction of the surface current, appears similarly in this and in the preceding equation, we have approximately †

$$\theta - (\psi) = 0.$$

Or, *That the average direction of the isobaric lines and the mean direction of the mass of air in motion are approximately the same.*

Cause of the difference between the direction of the motion of the upper and lower currents.—In the preceding investigation we have been occupied simply with numerical relations following directly from observations made without reference to any hypothesis. It will be easy, however, to show that the results obtained relatively to the different directions of motion of the atmospheric currents are in strict conformity with the reasoning of writers on this subject, from Hadley to Herschel.

Putting aside altogether the hypothetical ascending and descending currents of air, which were first suggested by Hooke, and for which we have no evidence whatever, we owe to Hadley the first indication of the

* If we consider the mean value of $(\psi) - \phi$ for each stratum to be half that at the upper and lower limits (it should probably be more than half), and assume that for the stratum above the cirri to be 32° ($\psi - \phi$ for the cirri being 29°), we have—

Soil to scud and cumulus stratum.....	= + 7.25
Scud and cumulus to cirro-stratus.....	= 18.65
Cirro-stratus to cirrus.....	= 26.20
Cirrus to upper limit of motion.....	= 32.00

and give to each equal weight, we find the mean,

$$(\psi) - \phi = +21^\circ.$$

There is no hypothesis that can be proposed, founded on the observed differences, which could affect the final results to any considerable extent, nor the conclusion which is deduced from it.

† The difference $(\psi) - \phi$ deduced from the observations at Makerstoun, confirmed approximately by those at Brussels, being supposed to hold at Greenwich and Dublin.

fact, that if a mass of air changes its latitude, from whatever cause, it will not proceed in the direction in which it was originally propelled or drawn; but if it passes to a higher latitude the motion will be towards a point more easterly, and if to a lower latitude towards a point more westerly than the original direction; that is, in our hemisphere the ultimate direction of motion will be *positive* of the original direction.

The friction of the moving mass of air against the earth's surface is always recognized as a cause which diminishes the amount of this displacement. Thus Hadley, on the lower currents from the tropics towards the equator:—"Before the air from the tropics can arrive at the equator, it must have gained some motion eastwards from the surface of the earth or sea whereby its relative motion will be diminished"*. The upper current moving towards the north is supposed by him to preserve its excess of velocity till it descends. So Sir John Herschel on the same hypothetical upper current:—"In flowing over to regain its level, it commences its course relatively in a meridional direction, but really with the full amount of easterly velocity which the earth's equator has; and since this, as it proceeds north or southwards, is in excess of what would suffice to keep it on the same meridian, it continually deviates to the westward [*i. e.* the direction from which it appears to proceed]; and when it again returns to the earth in its circulation, which it does on both sides beyond the tropics, it does so with a powerful westward tendency, *and the more, as in its course it has been less under the influence of surface friction owing to the elevated region in which it has travelled*"†. I have put in italics the explanation of the difference of directions of the currents found by me.

The theory of the trade-winds is so mixed up with unknown currents, ascending and descending, and presents so many other difficulties as usually stated, that it is not easy to determine what may be due to differences of latitude velocity, and what to other causes in the production of winds with definite directions in different parts of the globe. It is not a little remarkable, however, that no writer, as far as I am aware, had perceived that if the difference of latitude velocity affected the direction of motion of a vertical mass of air, this fact should be observed at once, especially in middle latitudes, in the movements of the wind and of the clouds.

If, as Sir John Herschel shows, the *lower* stratum of a mass of air moving northwards has its tendency to move eastwards most diminished, this diminution will be communicated gradually by the viscosity of the air from layer to layer till the upper stratum of the mass in motion is attained where "the influence of the surface-friction" is least‡. We

* Phil. Trans. 1735, p. 61.

† Meteorology, by Sir J. Herschel, 1861, p. 59.

‡ It is always understood that friction diminishes the velocity in all directions; but the cause which propels or draws a mass of air from one latitude to another is probably continuous for some time at least.

have here at once an explanation of the difference of directions found by me, and a proof of the conservation of the excess (or defect if proceeding towards the equator) of the initial latitude velocity; that is to say, another proof of the earth's rotation.

It is obvious that this explanation refers only to the mass of air set in motion by the same cause; it is probable, however, that the lower and upper strata are frequently propelled in different directions. This is evident in the case already noticed of the north-east winds and cirrus currents (footnote, p. 531); and is shown without doubt in the cases for which the upper currents proceed from points *negative* of the lower currents. I may also remark that the difference of direction of these currents will depend on the nature of the surface and on the difference of latitudes of the place from which the air originally started and the place over which it is moving. It is probably due to this cause that the difference $(\psi) - \phi$ is found greatest for winds from the southern quadrants; but this difference and its relation to θ will be considered in a special investigation.

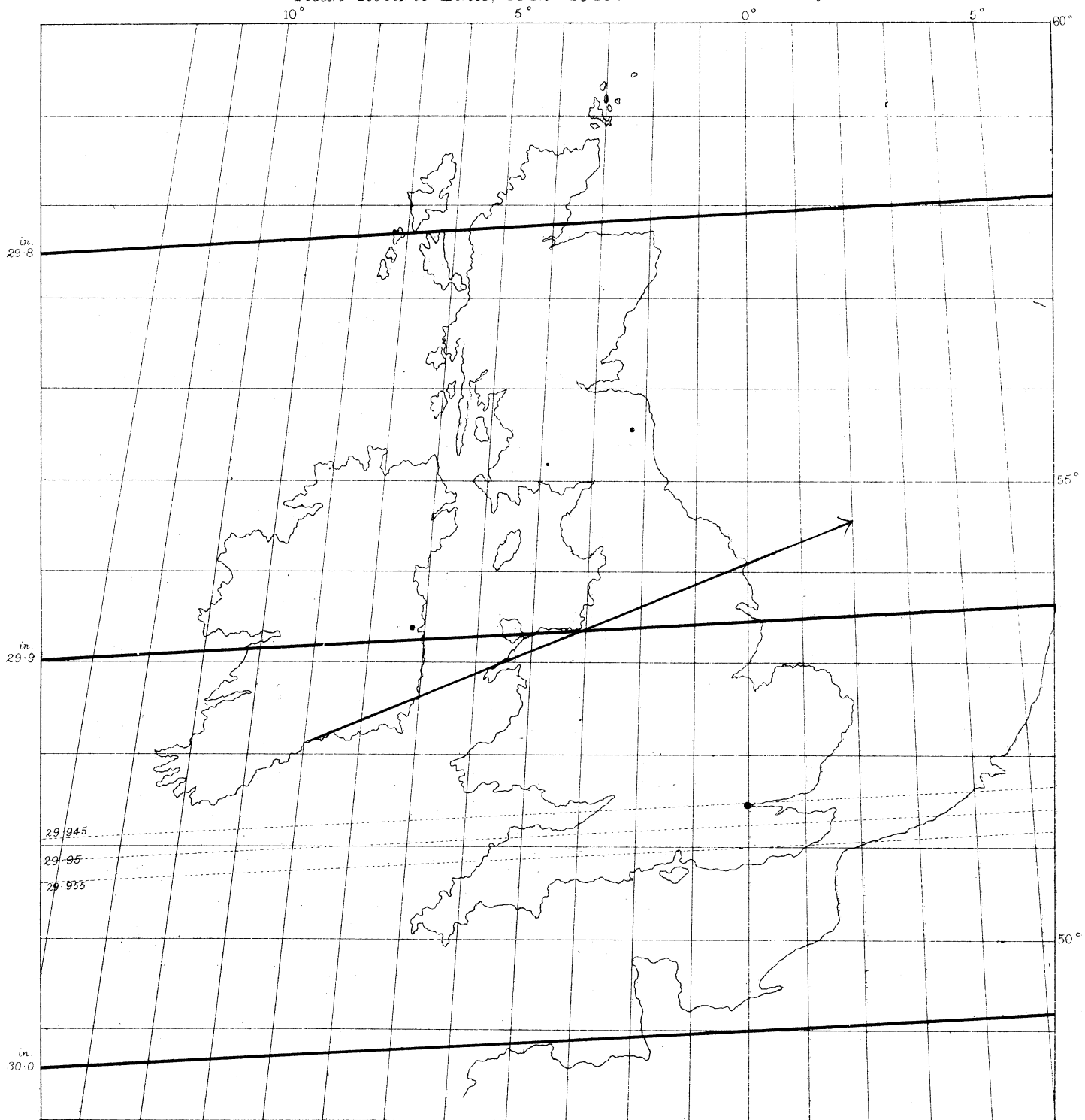
It is not my intention at present to enter into the many consequences which follow from the facts developed in this paper. It is evident that they indicate a wholly new theory of the circulation of the atmosphere. It was believed that the air was continually flowing into atmospheric basins; and as the barometer gave no indication of the fact, it became a logical necessity, as the air could not flow out below, that it must rush up above and flow off in still more rapid currents in directions for which there was neither law nor reason. It appears that the air does not pour into these basins, but that it moves as a whole in the directions of equal pressure.

Postscriptum.—I have remarked (see footnote, p. 526), on Mr. Buchan's authority, that Dr. Buys-Ballot had found the direction of the wind relatively to that of the isobars to be between 10° and 30° , giving an average of 20° . Mr. Buchan informs me (in a letter dated Dec. 27, 1876) that this result was communicated to him by letter. I owe also, at the same time, to the eminent Scottish meteorologist the communication of a similar investigation by the Rev. W. Clement Ley, some of whose results I am glad to be able to add to this paper.

Mr. Clement Ley has projected the isobarics obtained from the observations for one hour made at 15 stations, telegraphed daily: by means of a graduated circle he obtained approximately the directions of the isobars at each place (for three months nearly). These directions were then compared with the telegraphed wind directions, which were given to two points of the compass only; he found the average inclination of the wind to the isobar $(\theta - \phi)$ to vary at the different places from $+5^\circ$ to $+41^\circ$, and that the mean of the whole gave

$$\theta - \phi = +21^\circ \text{ nearly.}$$

Considering the many sources of error in the telegraphed observations,



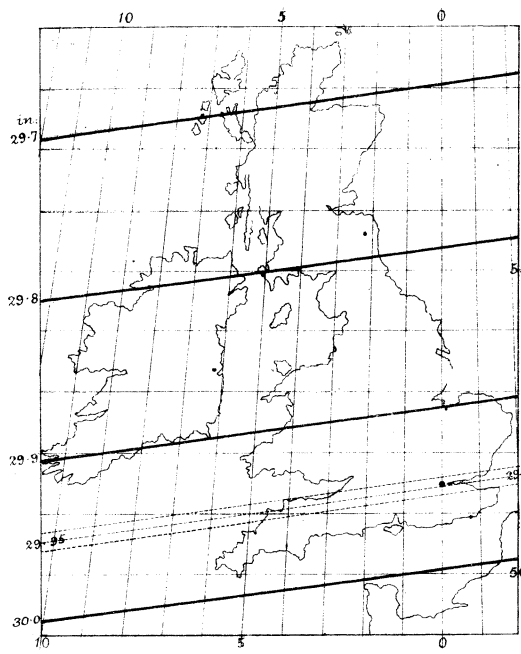
— Year. —

W. West & Co. lith.

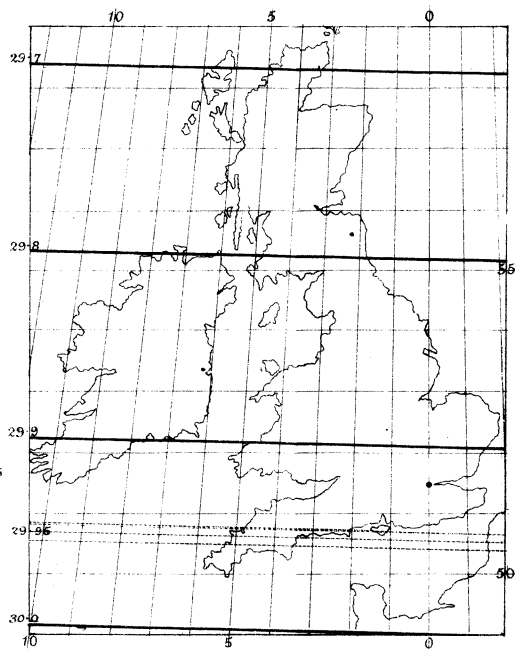
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Broun

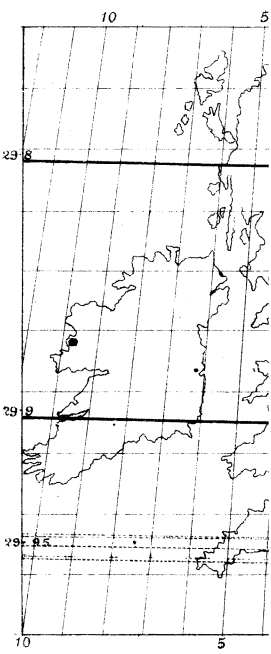
Monthly



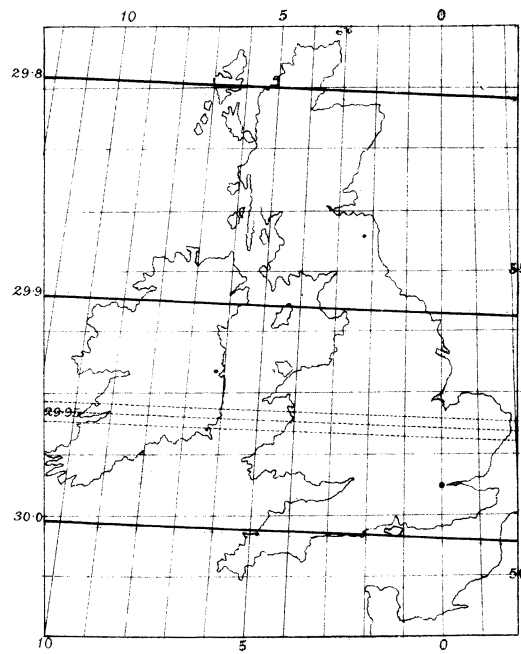
January.



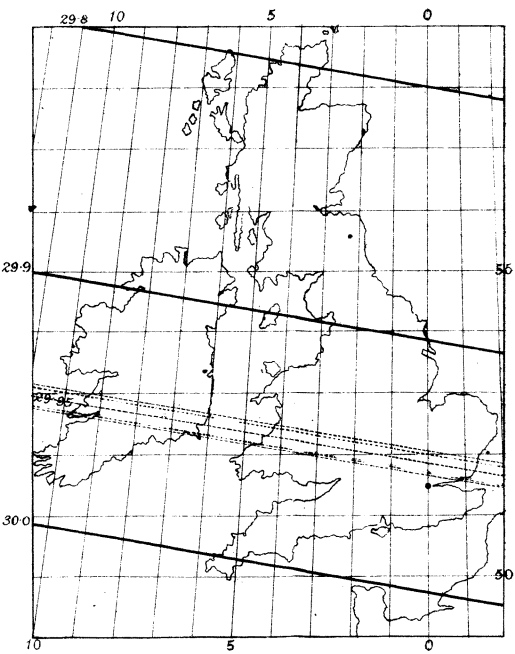
February.



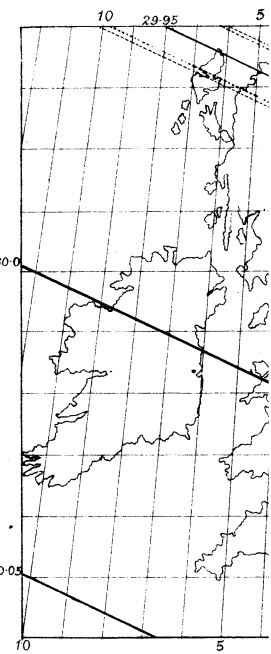
Mar



July.



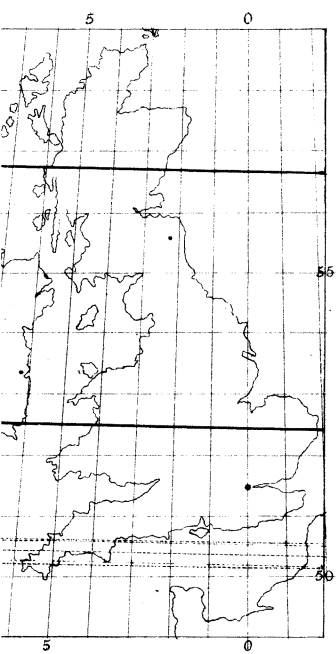
August.



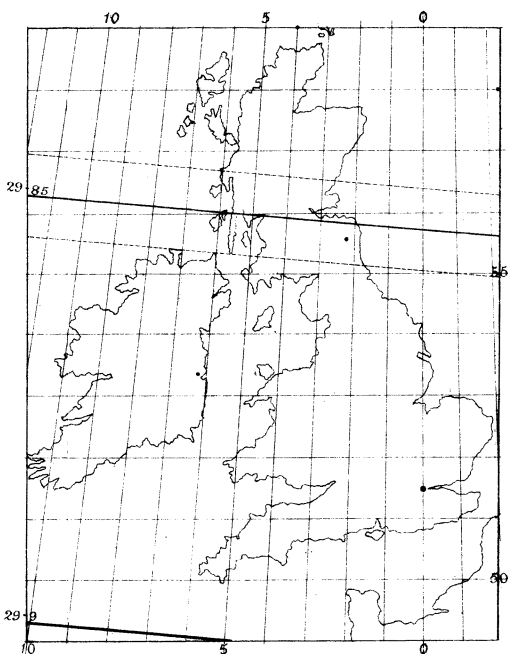
Septem

Vertical and Ho

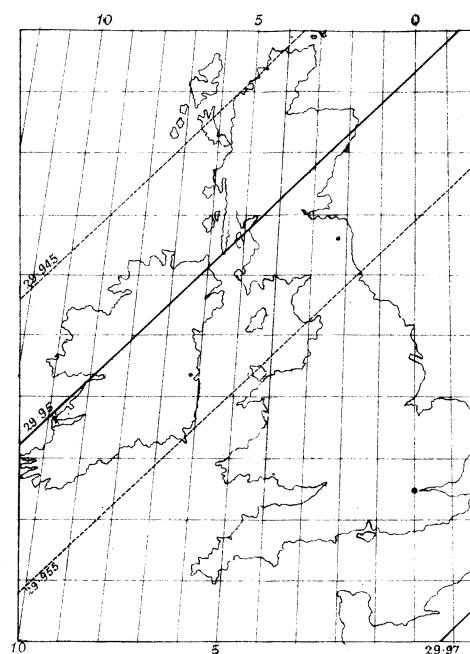
Monthly Mean Isobaric Lines, 1842 - 1849.



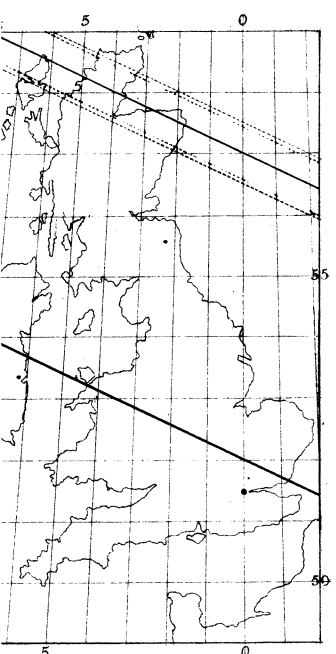
March.



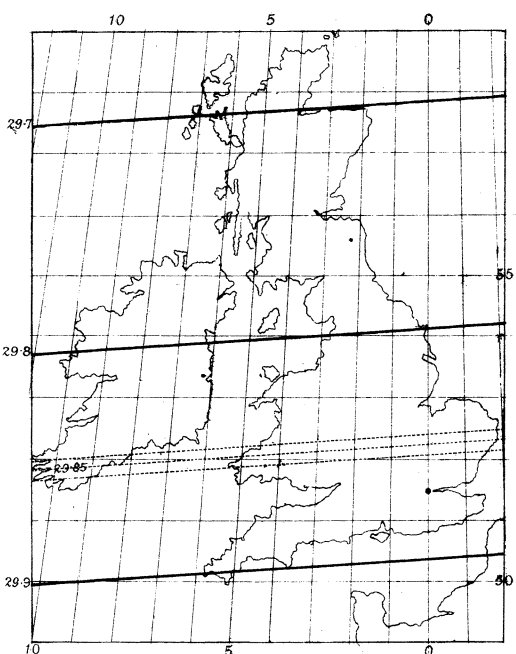
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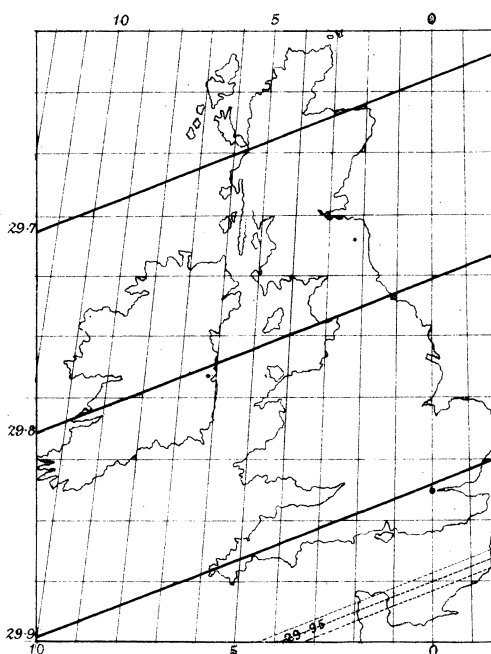
May



September.

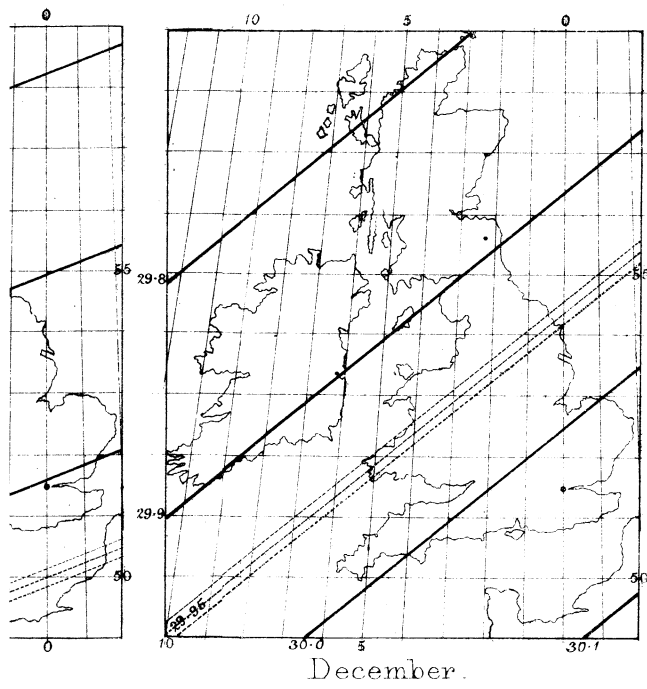
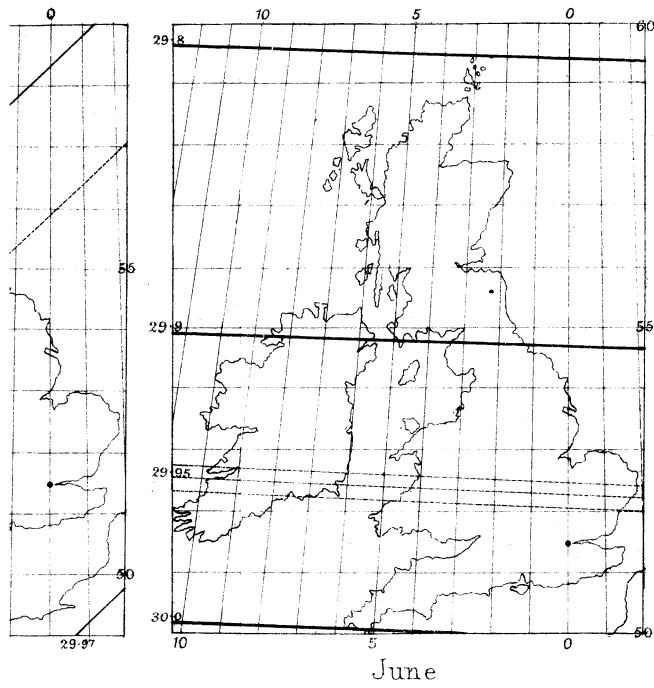


October.



November.

and Horizontal Scale : 0.005 inch = 1 Geographical mile.



especially in those for the direction of the wind at a single hour, the agreement of the final result with Dr. Buys-Ballot's mean (obtained probably in the same way), and that found here by a wholly different method, is very satisfactory.

One of the most interesting parts of Mr. Clement Ley's paper is the confirmation given, by so zealous and accurate an observer, of the results relating to the different directions of the aerial currents discovered by me upwards of thirty years ago. The author remarks,—“From many thousands of observations made at well-exposed situations in the Midland Counties of England, I have found that in about seven cases out of eight, with a light air or moderate wind, if the observer *exactly face* the wind on the earth's surface, he finds the current in clouds of no great altitude overhead to be a little *from his right*. This difference, it may be remarked, is commonly greatest and most uniformly noticed in dull or rainy weather with S.W. or S.E. winds”*.

That is to say,

$$\psi - \phi \text{ is positive,}$$

and most so in the south quadrants. These results are in exact conformity with those obtained from the Makerstoun observations†.

Mr. Clement Ley also finds the difference of direction between the isobars and wind to be greatest in the south quadrants (a result which I had proposed to examine specially, p. 538); but he does not seem to have remarked the important fact of the coincidence in the increase of $\theta - \phi$ and $\psi - \phi$. I recommend Mr. Clement Ley's instructive paper to the attention of meteorologists.

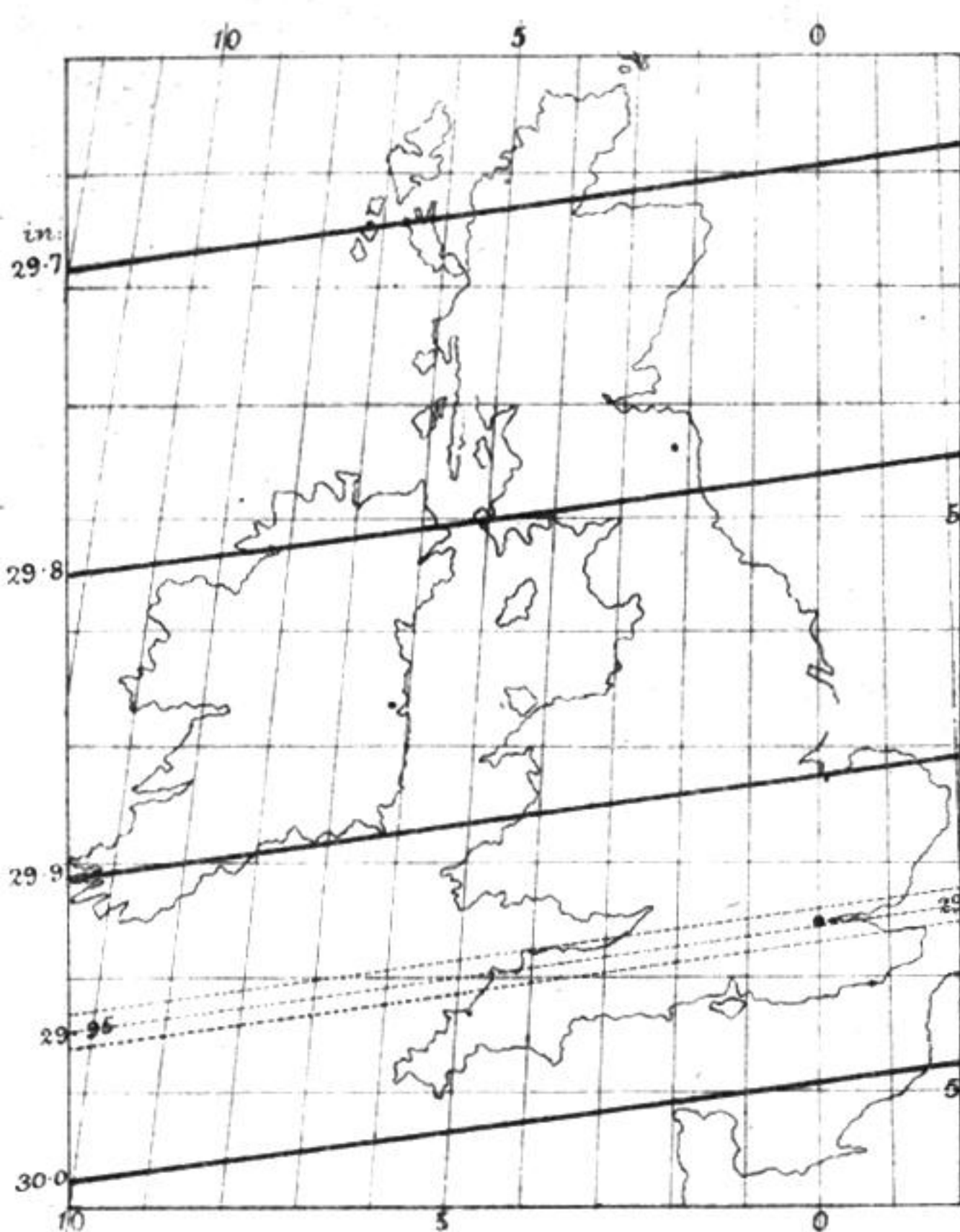
II. “The Meteorology of the Bombay Presidency.” By CHARLES CHAMBERS, F.R.S., Superintendent of the Colába Observatory. Received January 2, 1877.

(Abstract.)

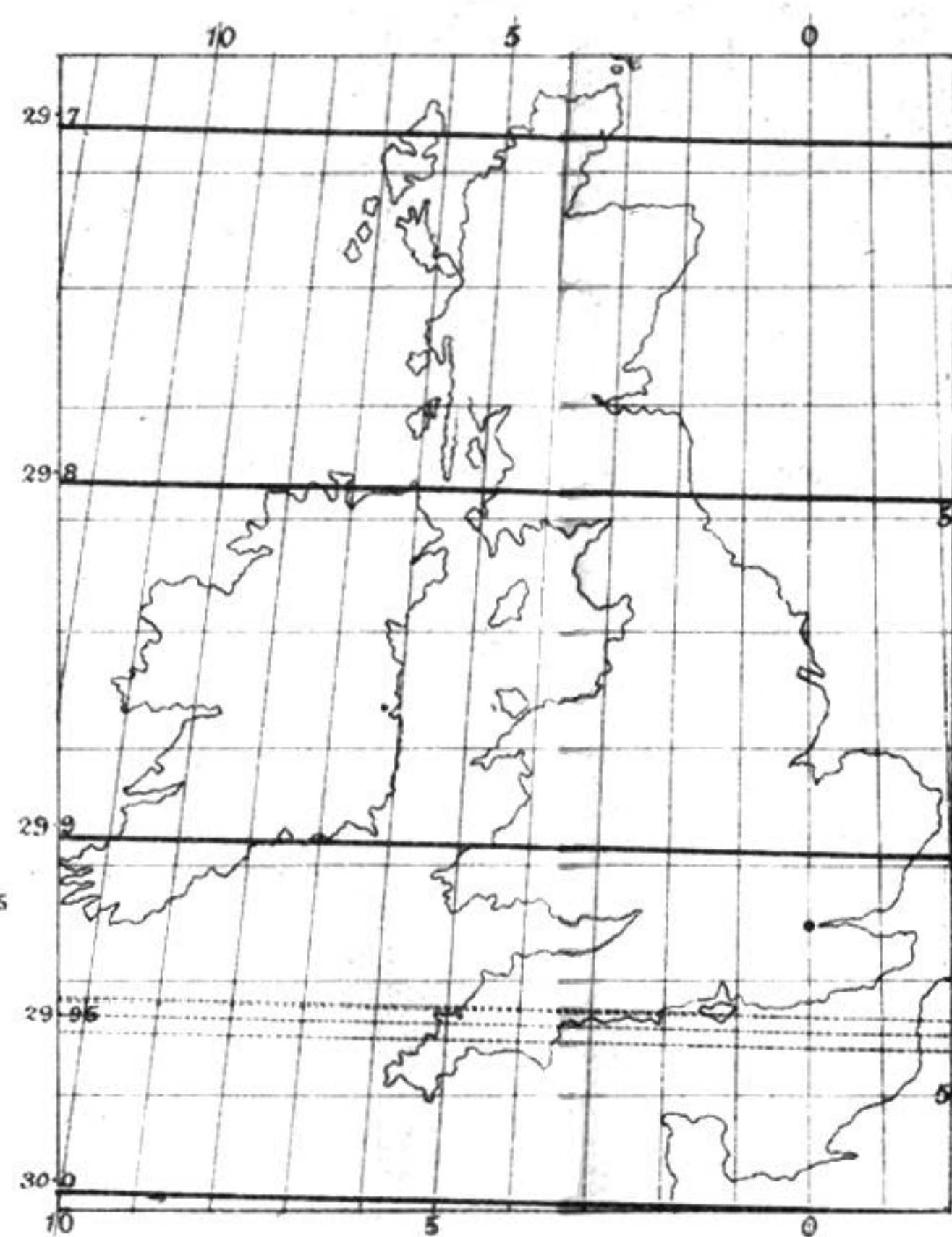
This work consists of four parts—the first dealing with registrations of meteorological phenomena at the Colába Observatory during a period of twenty-seven years; the second with moderately full observations at five military stations in the Bombay Presidency during a period of nineteen years; and the third with large numbers of observations from civil hospitals and revenue stations, being those of selected registers extending over various periods from not less than a fortnight up to a number of years: in this part the phenomena treated are temperature of the air, winds, and rainfall only; and the extent of territory to which the observations refer includes the whole of the Presidency, Sind, and the western

* Journal of the Scottish Meteorological Society, July 1873, p. 70.

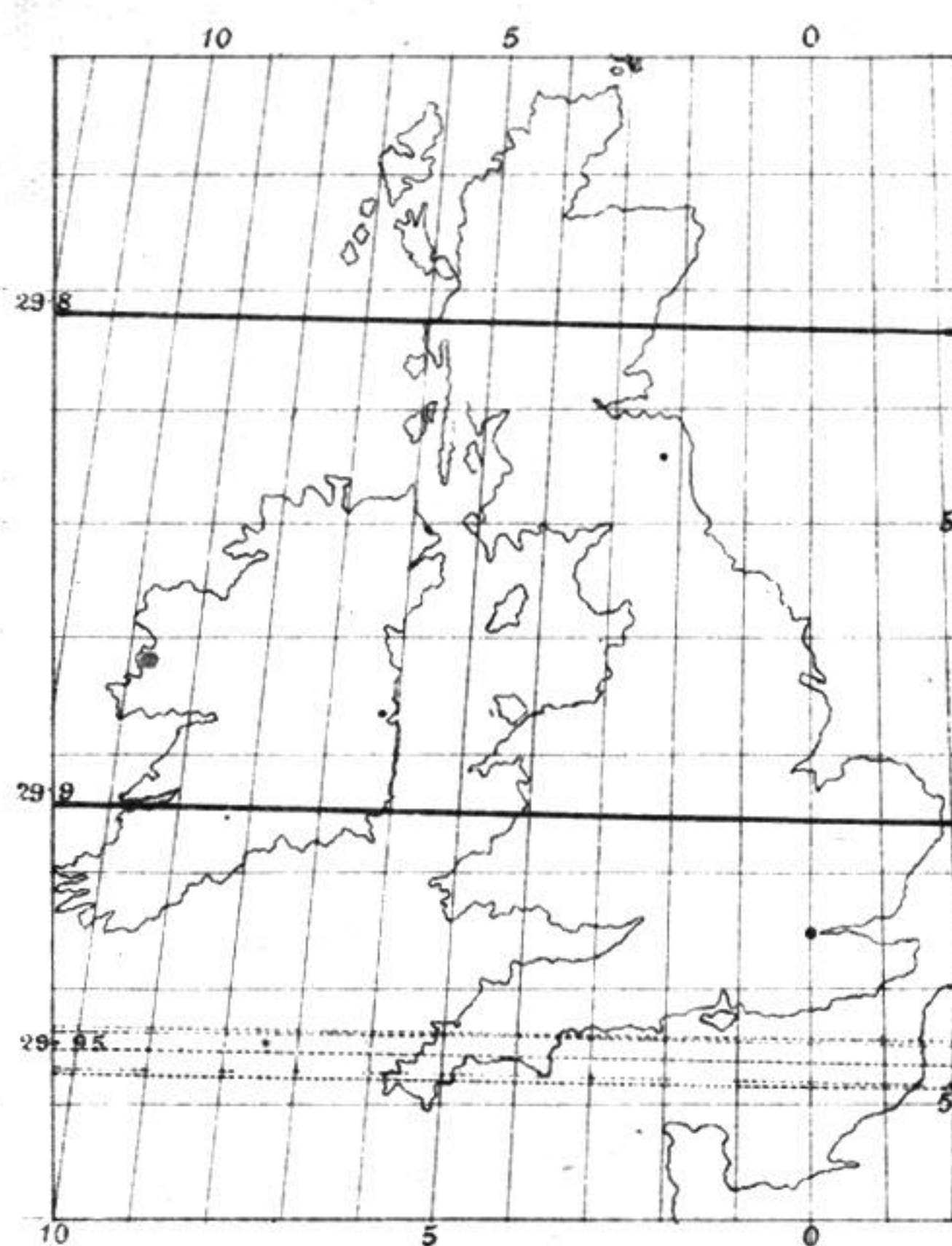
† “Results of Makerstoun Observations,” Trans. Roy. Soc. Edinb. 1843 to 1846, pp. ciii, civ.



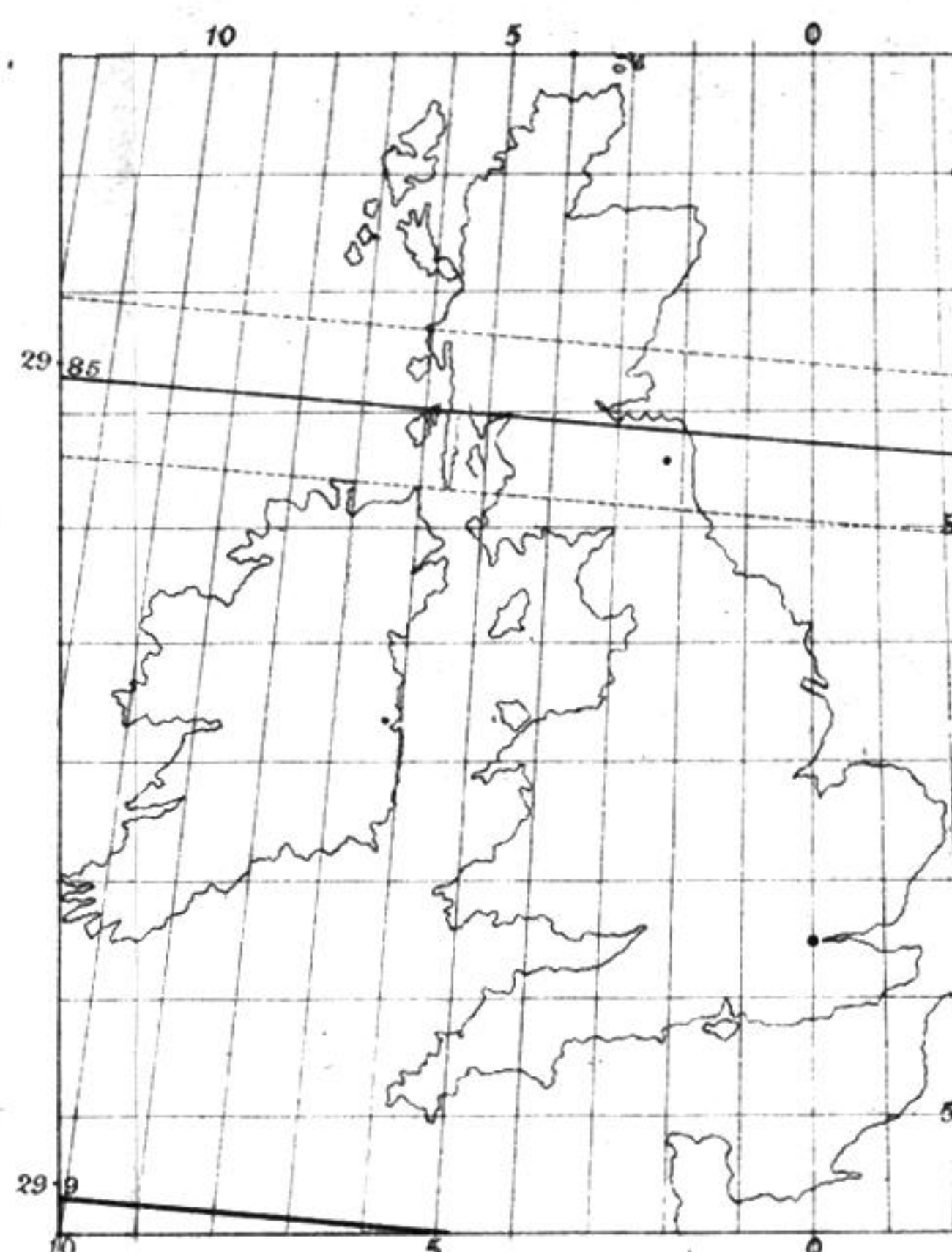
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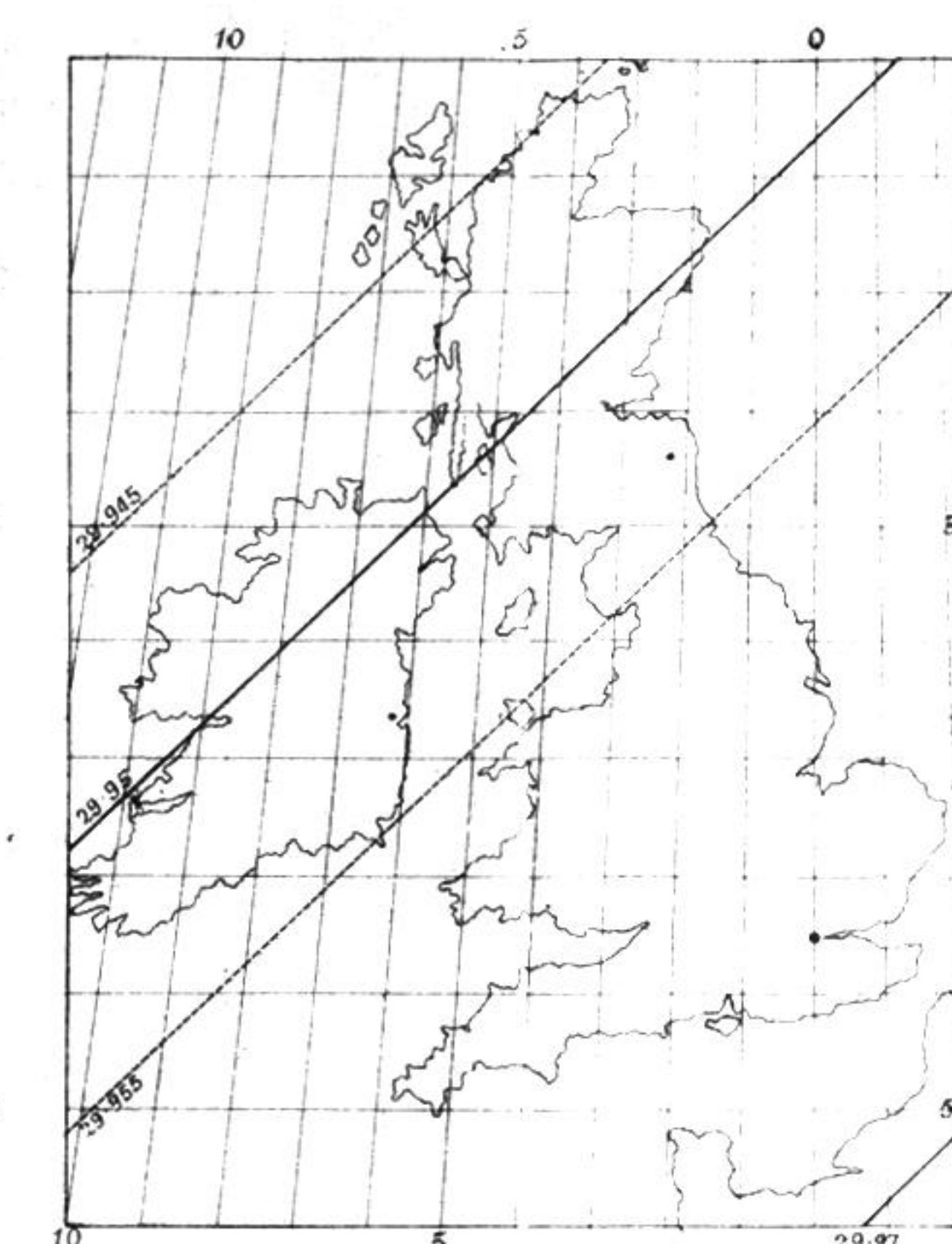
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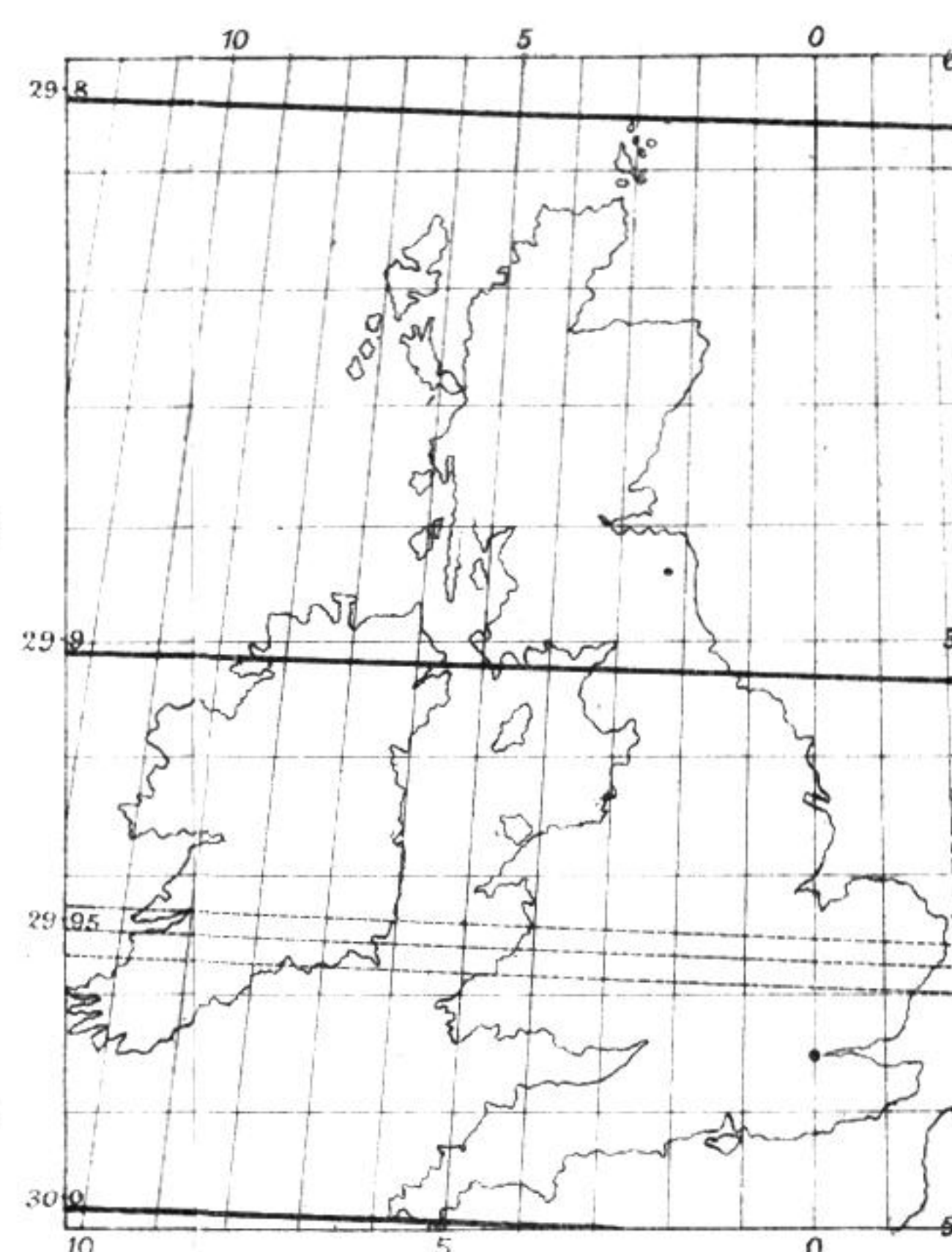
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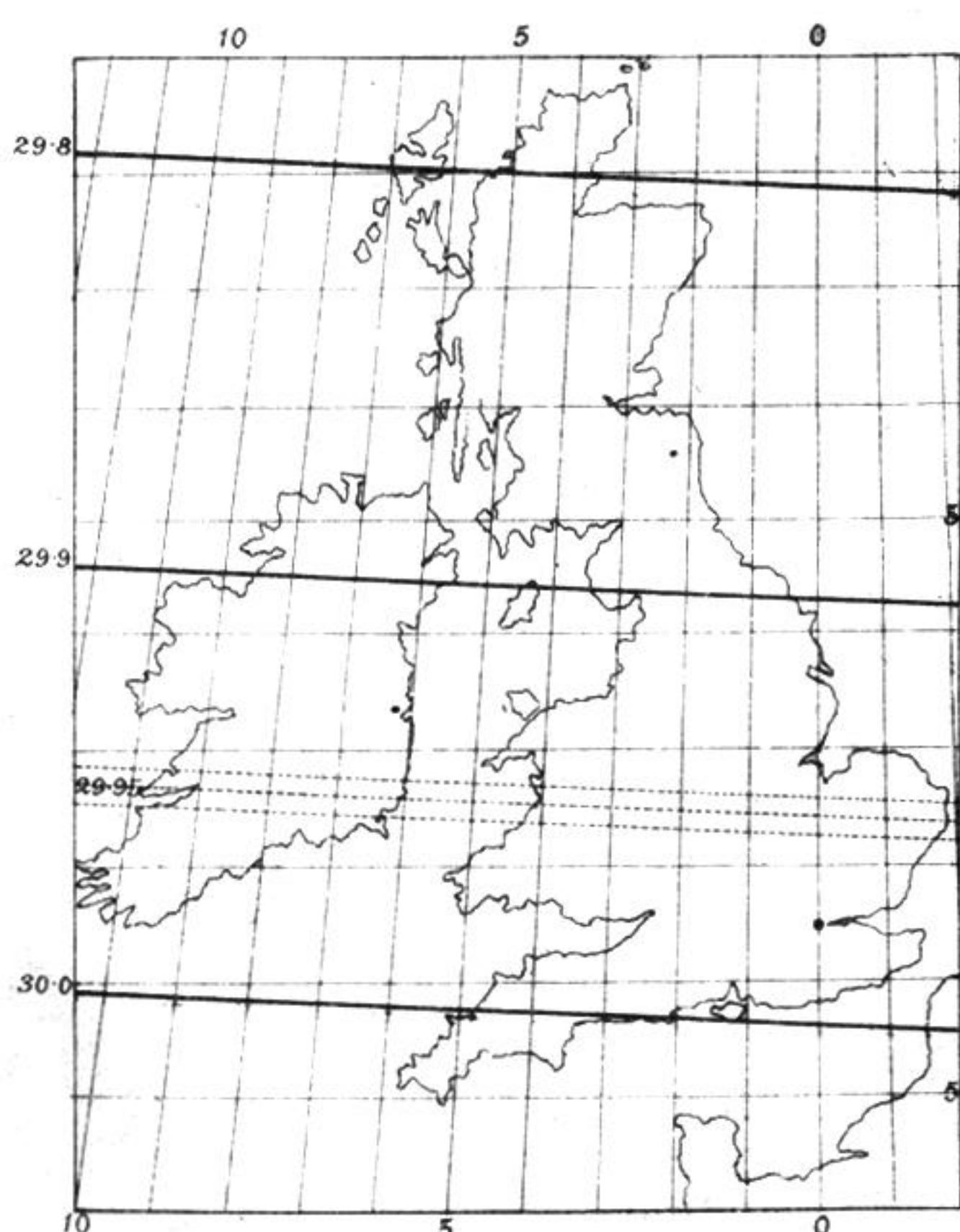
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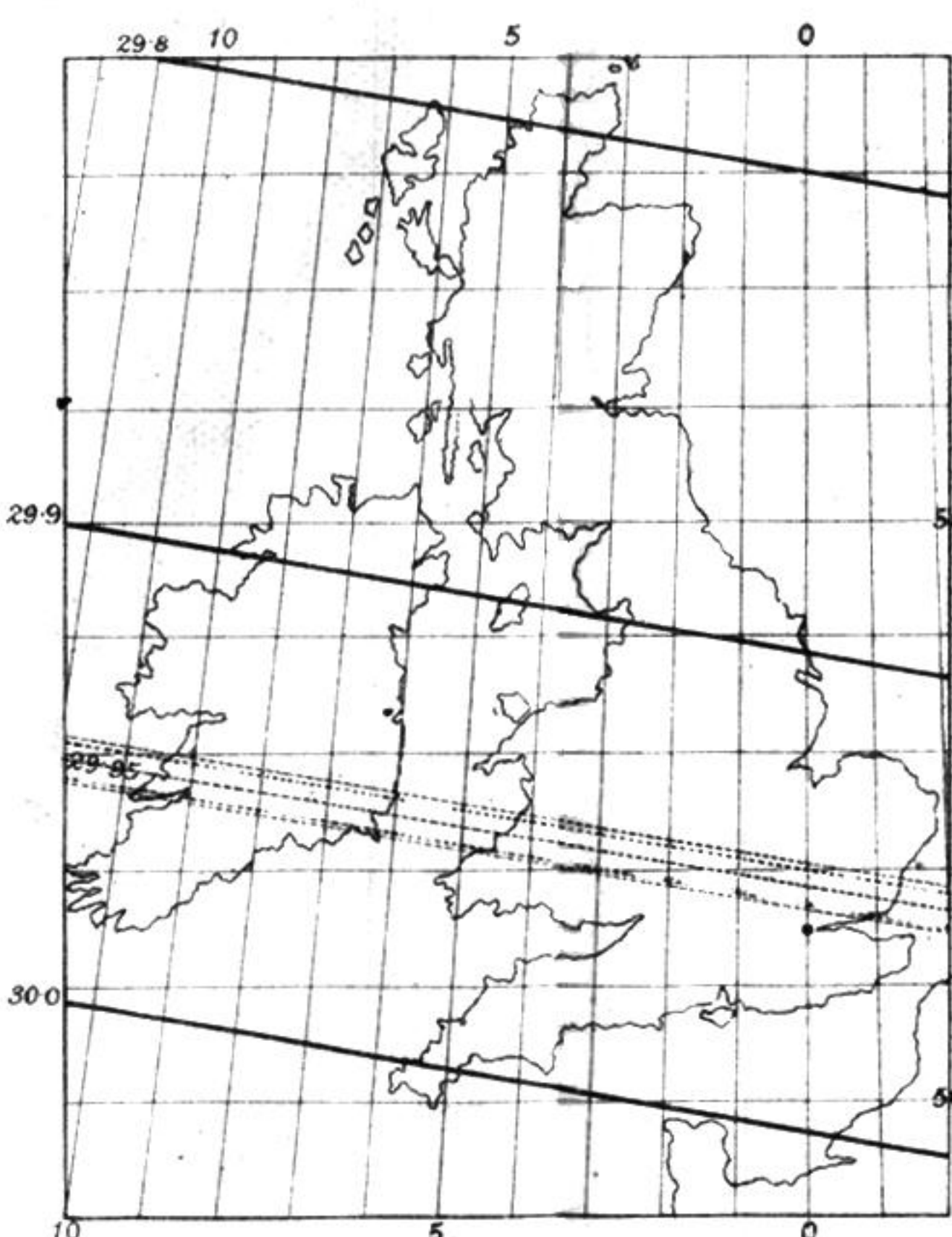
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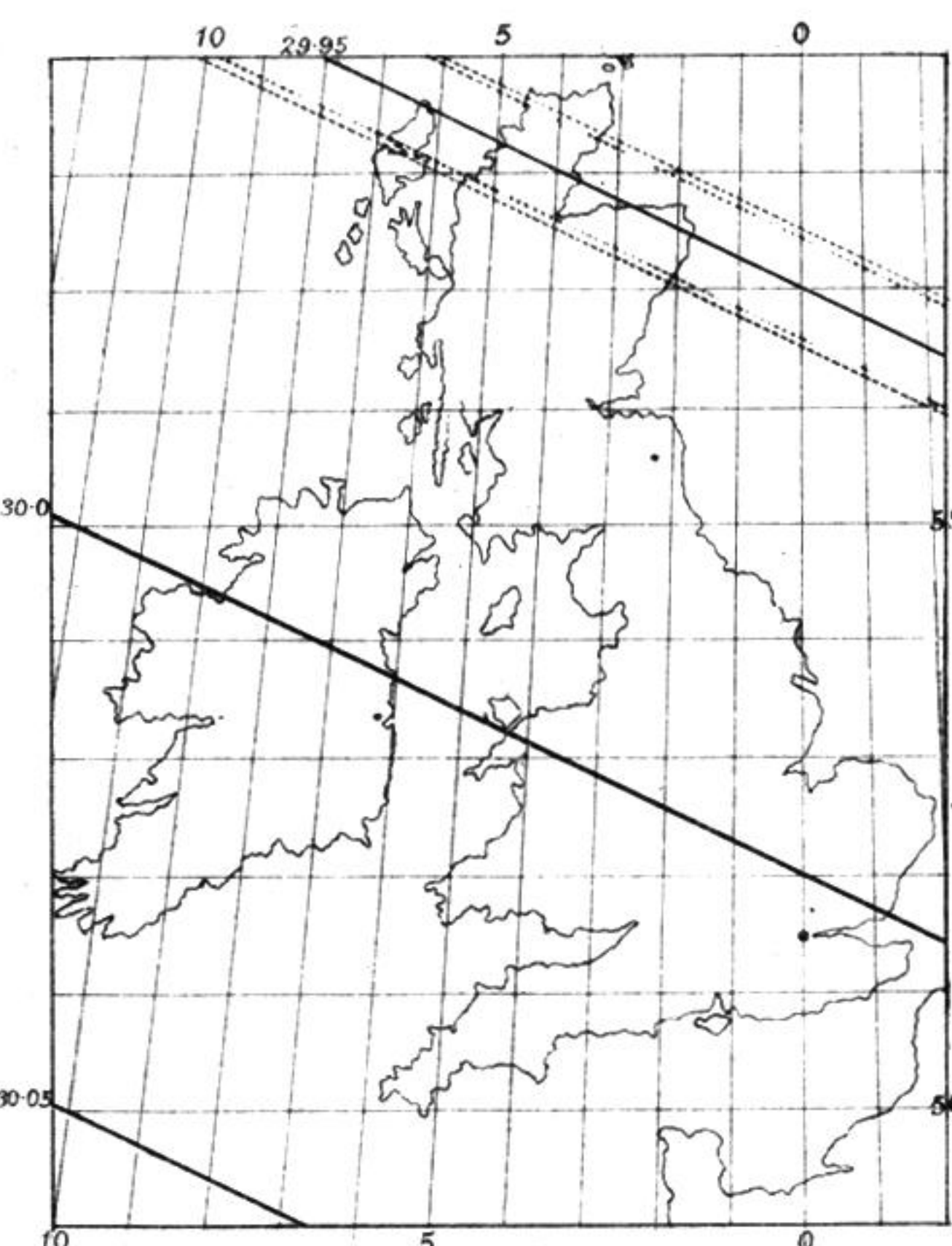
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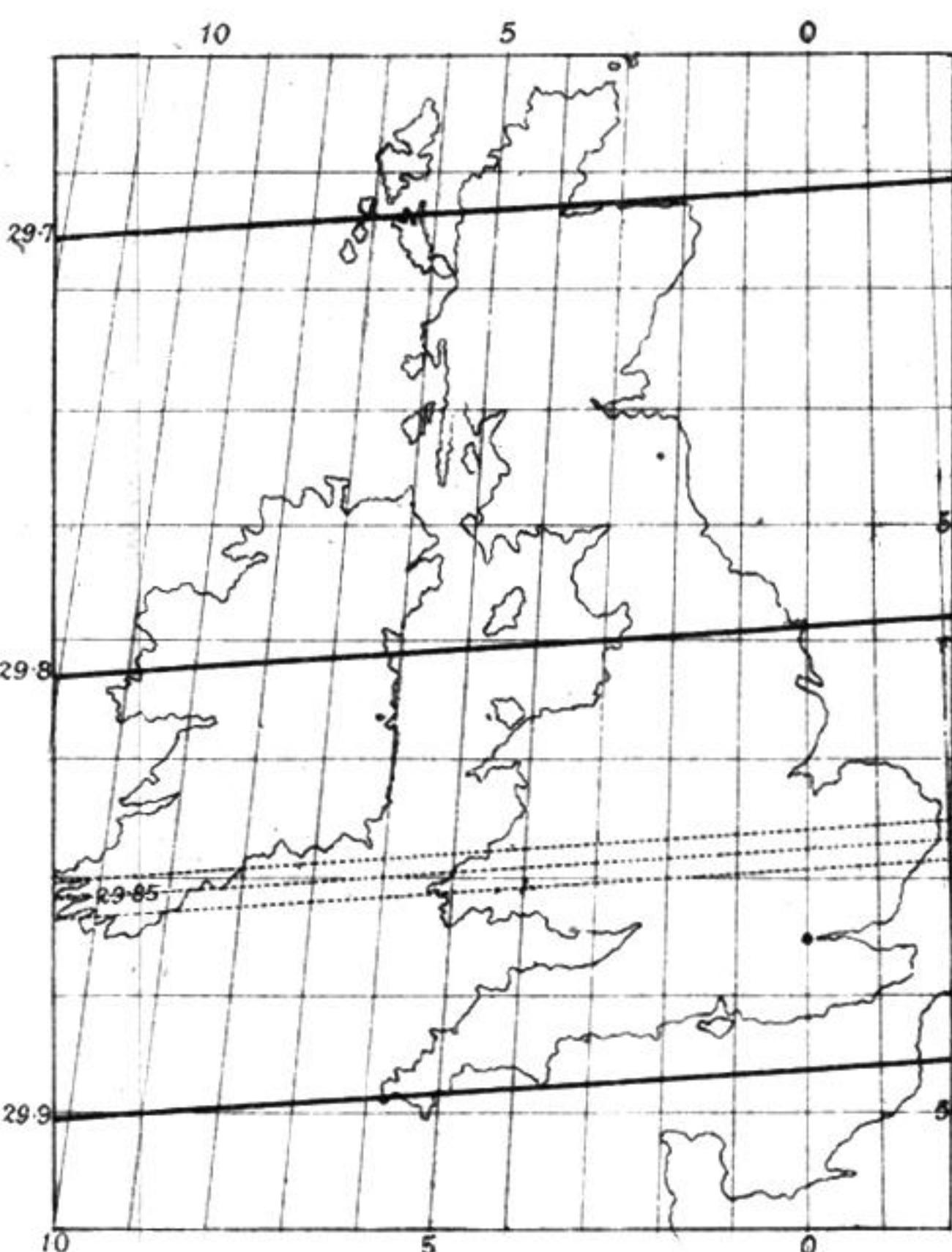
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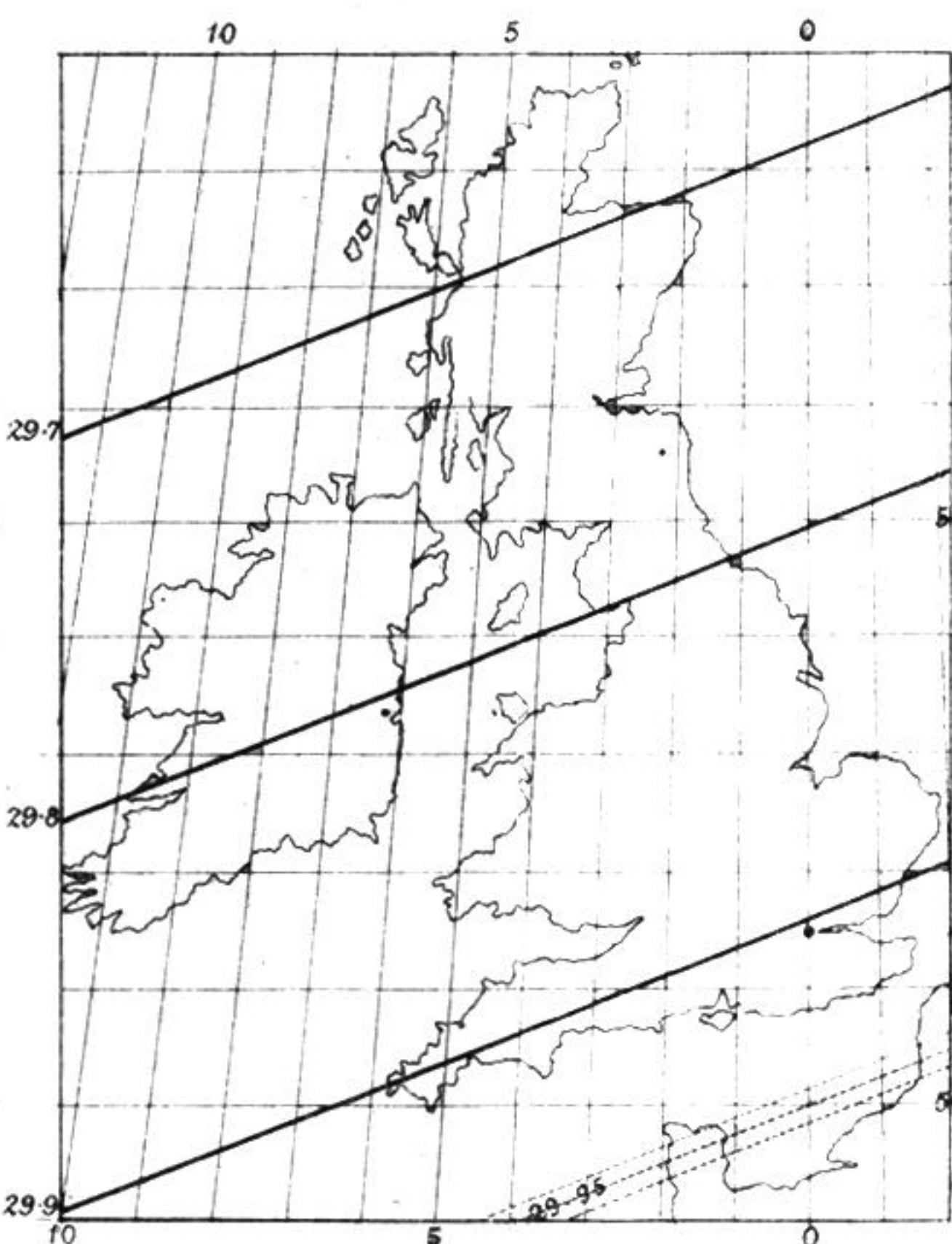
August.



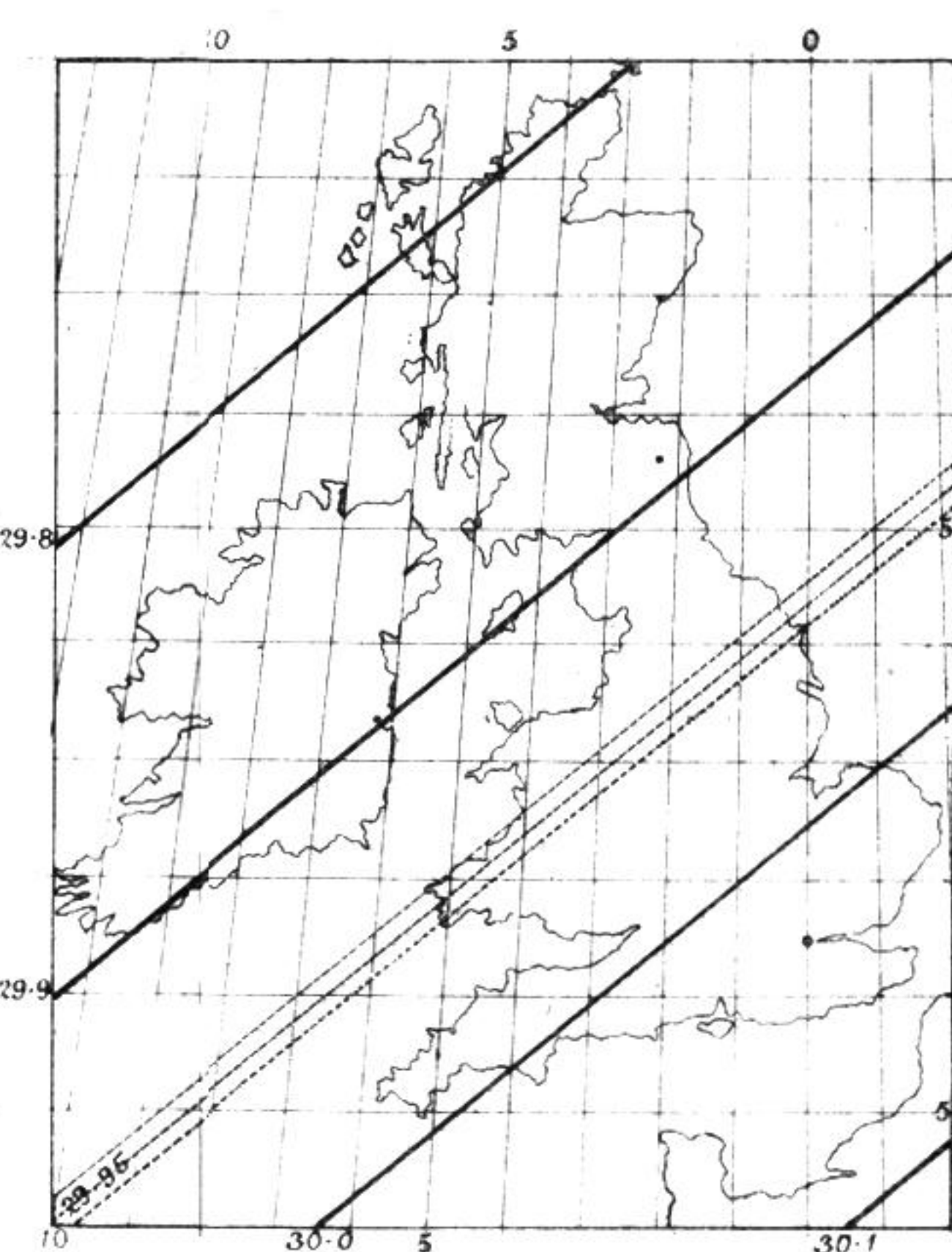
September.



October.



November.



December.

Vertical and Horizontal Scale : 0.005 inch = 1 Geographical mile.